By the same author

AN INTRODUCTION TO INDUSTRIAL RHEOLOGY

A SURVEY OF GENERAL AND APPLIED RHEOLOGY

MEASUREMENTS OF

MIND AND MATTER

by

G. W. SCOTT BLAIR

M.A. (OXON.), PH.D, D.SC.(LOND.), F.R.I.C., F.INST.P.

National Institute for Research in Dairying,
University of Reading

London

DENNIS DOBSON LTD

First published in Great Britain in MCML by DENNIS DOBSON LTD, 12 Park Place, St James's, London SW1. Printed in Great Britain by the BOWERING PRESS, PLYMOUTH.

137/R

This little book is dedicated to all those
who are not afraid to cross
frontiers

Who hath measured the waters in the hollow of his hand, and meted out heaven with the span, and comprehended the dust of the earth in a measure, and weighed the mountains in scales, and the hills in a balance?

Isaiah xl. 12.

Contents

Diagrams

* My own line drawings

Acknowledgments

No. 3. H. Hoagland. J. Gen. Psychol. 9, 267 (1933) Fig. 3.

No. 4. G. Herdan, Brit. Sci. News 1, 12 (1947) Fig. 2.

No. 6. H. J. Eysenck *Dimensions of Personality*, Kegan Paul (1947) Fig. 19.

Preface

I SUPPOSE IT IS HARDLY surprising that I should be interested in the Theory of Measurement. For twenty years I have been trying to measure queer things like the tilth of a soil, the strength of a flour and the body of a cheese, and my wife as a child psychologist is continually measuring 'intelligence' and similar concepts.

Recently, moreover, I have for some time been tinkering with the theory of dimensions in my own field of rheology and have been criticized for doing so. Whether these criticisms are justified is not for me to say but, naturally enough, it had not failed to occur to me, as soon as I saw where any satisfactory explanation of the data from my experiments was leading me, that it would never do to tinker with dimensions without first making a fairly thorough study of the classical theory, so I made something of a hobby of the subject.

Meanwhile, in accompanying my wife to meetings of the British Psychological Society (which I later joined as a 'lay member') I came across a number of interesting people who appeared to be struggling with dimensional problems in Psychology very similar to my own in Physics.

My friend Dr Robert Silver suggested that I should put down some of my ideas on the problems in book form and I have done my best to comply. The danger in writing a short and not too technical book, however, on a subject that covers rather a wide variety of fields, is that one may inadvertently be guilty of inaccuracies, especially when describing experiments with which one is not directly familiar. I have tried to avoid this and, if I have not failed too badly, this is due to my friends Dr R. Silver, Prof. M.

Reiner and Mr R. Harper, who have read the typescript and, though in no way responsible for my views, have corrected a number of errors which I had made as well as making valuable suggestions. Other friends, too numerous to name, have allowed me to pick their brains and, if they should read this book, they may recognize the pickings. They are, of course, included in the dedication.

I would also thank Mrs Steer for never complaining when I have spoiled the beauty of a neatly typed page as a result of all too frequently changing my mind.

It does not seem desirable in a book of this kind to include a large number of references to scientific papers and, with a few exceptions, I have therefore quoted only standard text-books from which the full bibliographies can be obtained.

The extension of the Theory of Quasi-properties and other ideas of mine requires experimental tests with a variety of materials apart from those few available to me in the dairy industry and I therefore dare to hope that some readers of this work will feel disposed to try to extend or disprove the ideas I have put forward by experimenting with their own materials, and in branches of physics other than rheology.

But I also hope that there are others who are not scientists who may be interested in what the scientist is trying to do and I have tried to make the book understandable to such readers.

G. W. SCOTT BLAIR

NATIONAL INSTITUTE FOR RESEARCH
IN DAIRYING,
UNIVERSITY OF READING

April 21st, 1948

Postscript

As a result of circumstances beyond the control of the author and publishers, the publication of this book has been much delayed. Through the courtesy of the publishers, I have been

allowed to make a few corrections and amendments before the book finally goes to press.

So much new matter has been published during the last eighteen months that I should have liked to re-write much of the book; but this is clearly impossible and it must go forward, as most other scientific books now published in this country are alas also going forward, in a patched condition.

Were I writing the book now, I should not fail to mention L. L. Whyte's much discussed *Unitary Principle in Physics and Biology* (Cresset Press, 1949) nor E. A. Milne's great work *Kinematic Relativity* (Clarendon Press, 1948). A recent paper by P. Moon and D. E. Spencer (*Amer. J. Phys.*, *17*, 171, 1949) would also have been discussed, as well as recent developments arising from work in my own laboratory.

The death has recently been announced of Mr J. W. Dunne, whom I did not have the privilege of meeting: I should like to have had his views on my comments on 'Serialism' but this was not to be.

I have tried, then, to the best of my ability, to let it be clear where the views expressed are those of April 1948 and where they have been modified by later events. I hope that I have not altogether failed in this respect.

Much of this book has had to do with the problems of comparing the values of incommensurables: dare one hope that scientific books, however humble, will some day be given a place in the congested line of priorities commensurate with the fact that, unlike certain other wares, such as cheese and wine, their value is apt to fall exponentially with time?

G. W. S. B.

23rd September, 1949

CHAPTER I

Introduction: What this Book is About

I WONDER HOW MANY people realize that scientists are by no means agreed among themselves on a definition of science? Many definitions have been proposed, from 'organized common sense' (though to the new-comer most scientific theories sound like uncommon nonsense) to Professor Dingle's definition[11]* which he has since discarded: 'the recording, augmentation and rational correlation of those elements of our experience which are common, or potentially common, to all normal people.'

We find similar lack of agreement on definitions for living matter: living things generally have certain characteristics in common but for borderline cases, such as the viruses, which have some only of these characteristics, there is no common agreement.

In spite of Dingle's later criticism[12]* of his own definition of science, which is made on philosophical grounds, I shall take it that the main points required for any adequate definition are to be found in his formulation. Science consists in recording and tabulating an ever-increasing number of observed facts and of linking these facts together. The group of facts in which science is interested is that which could be verified by anyone with normal faculties given adequate training and equipment.

Of course, such definitions lack precision. What are normal faculties? All scientific observations are finally recorded through the senses. Apart altogether from obvious sense-defects, what is

* References throughout the text are to authorities listed on pp. 113–114.

to happen, Dingle asks, when a certain chemical compound* is reported by some people to be tasteless and by about an equal number of others to have a strongly bitter taste? Who is normal? We are reminded of the alleged Quaker saying: 'All the world is queer, dear—except thee and me, dear—and even thee's a little queer, dear!'

We shall be discussing later the problems of matching colours, in which the 'just noticeable differences' vary for each individual. How far from the average must an individual prove to be for him to be regarded as abnormal?

We have become so much impressed, and rightly impressed, with the majesty of the order of the universe, with the clear-cut unity of things, that we have tended to forget the uncertainty of the outlines. There has been so much worthwhile to do and so much wanting to be found out in places in the middle of the structure, well away from the dangerous edges, that scientists have tended to view askance the problems on their own borders. It is true that atomic research with its emphasis on the Uncertainty Principle and its refutation of the simple mechanistic models of the Victorian Age has had its effect on this trend; but it has generally been felt that such limitations are concerned only with conditions of extreme smallness and extreme speed. Indeed such preoccupation with the atoms has tended to obscure the need for further work on the phenomenological behaviour of ordinary complex matter.

In psychology also, the tremendous field of work opened up by Freud's researches on the Unconscious has absorbed so much energy that developments in the psychology of perception have tended to be overlooked and the study of psycho-physics which covers the borderline of the ignored sections of both sciences has been largely forgotten.

Indeed, there is something almost disreputable about psycho-physics. Those of us who have tried to publish scientific articles about psycho-physical experiments find a strong resistance on the

*p-ethoxyphenylthiocarbamide. (The same is true of a number of chemically similar compounds.)

part of most physics journals and no welcome from some of those devoted to psychology. In the latter case, we are told, truly enough, that the average psychologist cannot be expected to understand physics; but in the former, the resistance would seem to be largely emotional.

Matters were not helped, of course, by some of the earlier psycho-physicists whose work has been thoroughly 'debunked' by such writers as Bergson.[3] I shall go into this question more fully later. At this stage I will only suggest that in science it is a mistake to visit the sins of the fathers upon the children and that the mistakes of the nineteenth-century psycho-physicists presumably serve as a warning to their successors in the twentieth century, who should therefore be less and not more likely to fall into the same type of snare. This tendency of scientists to develop 'preferred fields' and to despise work in the forbidden borderline territory, may well be one of the causes of the remarkable lack of popularity of science among the general public. Whatever the politics of the man in the street of this generation may be, snobbery is at a discount and I think that perhaps he feels, not without some cause, that there is too much intellectual snobbery among scientists.

We have only to look at the general standard of scientific journalism or at the amount of time allocated to science by the B.B.C. to realize how little scientific matters mean to the general public. The B.B.C. bases its choice of programmes on 'Listener Research' and, within the tiny fraction of its time devoted to science* it arranges not only for the best research workers to speak but, lest the scientist should often prove a bad advocate in his own cause, there are feature programmes in which professional actors and broadcasters use their skill to interest the public in science. Yet from the total time allowed to it, even by comparison with music, drama or literature, science is evidently of very little interest to the man in the street in spite of its obvious threats to his peace of mind or potentialities for the increase of his comfort.

* There has been a marked recent improvement, especially in the Third Programme.

No, science is technical, difficult and dull and is generally supposed to deal with abstract matters which have little connection with everyday life except, of course, in so far as the power which it brings increases our dangers or improves our amenities.

Of course, it would be as fatal to popularize science by compromising its principles as it has been to pursue such a course with religion. People will accept hard sayings not by compromising them but by making them meaningful. In our struggle towards what Lancelot Whyte calls 'the Science of Man"[49] we are held back by many things, not least by this idea of 'preferred fields'.

The preferred fields themselves suffer, moreover, like the Dominion of Pakistan, from consisting of non-contiguous territories so that scientific research tends to be sectionalized and, in university science especially, much that is of value falls between two Chairs.

This danger has not, of course, been missed by those who have the time and ability to see the wood in spite of the trees, but there is one aspect of the problem which I think must be tackled before much further progress is made.

One of the first essentials of science is the concept of quantity and the process of measurement. Classification is as necessary to science as is anatomy to medicine, but both are means to other ends. The mere collecting of plants, fossils or stars has now ceased to be a serious occupation, though Leonardo da Vinci could hardly have foreseen how long it would take the world to realize his conclusion that 'there is no certainty where none of the mathematical sciences can be employed or in the case of things which cannot be connected with mathematics.' You can as readily be a scientist with no interest in mathematics as a doctor without *materia medica*. The fear of mathematics current among many industrial scientists is a phobia for which the teachers cannot escape responsibility and it is indeed a heavy responsibility. (I do not know whether there exists to-day any feeling against pure mathematicians such as was expressed by Roger Ascham who wrote: 'Mark all Mathematicall Heades which be onely and wholy

bent to those sciences, how solitarie they be themselves, how unfit to live with others and how unapte to serve in the world!')

Mathematics itself has been largely concerned with what Wertheimer, one of the founders of the Gestalt school of psychologists, has called 'a summation of elements'.[46] As long ago as 1924, he asked: 'Is it necessary that all mathematics be established on a piece-wise basis? What sort of mathematical system would it be in which this were *not* the case? There have been many attempts to answer the latter question, but almost always they have fallen back in the end upon the old procedures. This fate has overtaken many, for the result of training in piece-wise thinking is extraordinarily tenacious. It is not enough and certainly does not constitute a solution of the principal problem if one shows that the axioms of mathematics are both piece-meal and at the same time evince something of the opposite character. The problem has been scientifically grasped only when an attack designed to yield positive results has been launched. Just how this attack is to be made seems to many mathematicians a colossal problem, but perhaps the quantum theory will force the mathematicians to attack it.'

I am no mathematician but, in a later chapter, I shall have a few suggestions to offer as to a possible line of approach.

Is reality, then, to be divided into the extreme duality of Descartes' *res extensa* and *res cogitans*? If we cannot measure the primary particles from which matter is constructed with the precision supposed possible by our forebears, may not perhaps the *res cogitantes* be less immeasurable than we had supposed? What really is meant by measurement and what are its true purposes and limitations? We had better look into these questions before we go any further.

CHAPTER II

Interlude: What is meant by 'Dimensions'?

I INTEND IN THIS short chapter to give a very elementary account of what is meant by 'dimensions', but I have written the rest of the book in such a way that the reader who is already familiar with the meaning of the term can skip this chapter altogether and continue reading from Chapter III without any lack of continuity.

We will suppose that we want to describe some event—say an item of police-court news. Suppose that Silas Q. Packenbocker is alleged to have hit Antonio S. Parelli on the nose in an apartment house in an American city. I choose an American city only because the American system of numbering streets and buildings is much more near to the scientific method of specifying location than is our own. 'The Laurels, Acacia Avenue' means something to the local postman, but is not very helpful to a stranger, as we all know only too well from experience!

To return to the alleged assault, the important points to establish would be: (1) Where did the incident take place? (2) At what time? (3) Other information such as 'how hard and just how was the blow delivered?'

I have drawn an idealized picture of the American city in Fig. 1, in which the black square represents the scene of the crime. We could describe the location as 'on the 17th floor of the apartment house on the corner of 3rd and C Streets, N.W.' (There might, of course, be four corner apartment houses, but you will see immediately that this does not alter the principles of my argument.)

In many State capital cities and in the Federal capital of Washington, streets are numbered more or less in this way. The 'blocks' need not all be of the same size—indeed, I have drawn them as

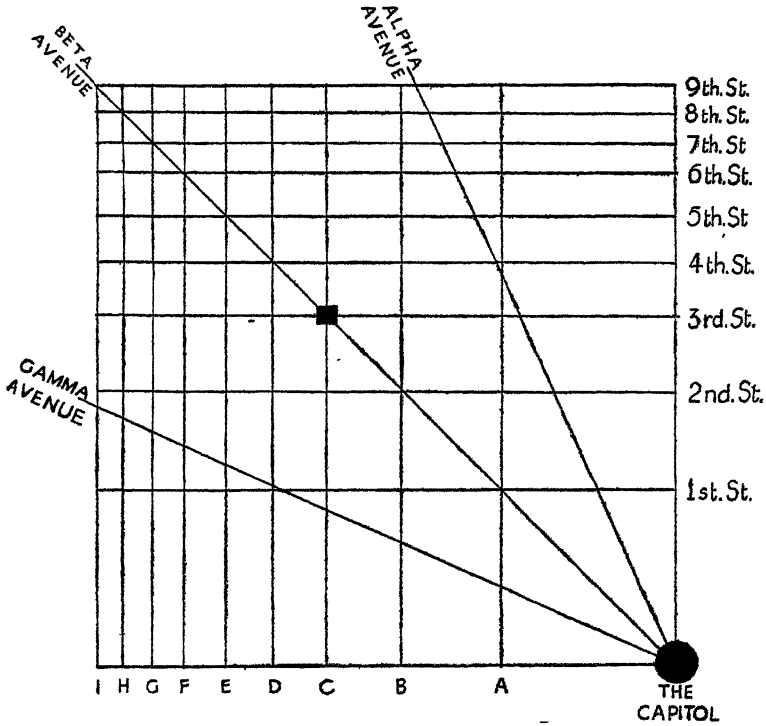


Fig. 1
Idealized Plan of Part of an American City

diminishing in size in a regular manner as we go further from the administrative centre—which is often approximately the case, though not in the manner shown.

It is clear that three magnitudes are required as well as a focal point or 'origin', as we call it, in this case, the Capitol, to describe the site of the incident. We require to know how many blocks north, how many west and, assuming a flat ground level, how many floors up. (If the ground were not level, we could, of course, specify the height in feet above the ground level at the Capitol or above sea-level.)

These distances are measured at right-angles to one another and are called rectangular or rectilinear co-ordinates. If the blocks

between the streets were all the same size, they would be called 'Cartesian Co-ordinates' after the French mathematician and philosopher, Descartes. We do not have to use rectangular co-ordinates. Some cities, including Washington, have Avenues running radially from the Capitol, generally called by names which do not indicate their location but, to simplify this, I have called the Avenues, 'Alpha', 'Beta' and 'Gamma' Avenues. We could equally describe the site as being a certain distance along Beta Avenue N.W. Co-ordinates of this kind are called 'Polar co-ordinates' but you will notice that the number of magnitudes required is always the same—three.

If we are not interested in height but only in the place on the map, we require only two magnitudes or if we are concerned with the distances along a particular road, only with one. These magnitudes are called the 'Dimensions of Space'.

Unfortunately, the word 'dimensions' is used also in other senses. First, there is the colloquial sense: 'Her dimensions were such that she found it difficult to get on to the bus.' Here the word really means 'size', or better 'size in different directions'. But the word also has another technical sense which differs somewhat from its use for 'the dimensions of space'.

We might describe the time as '3 p.m.' which means, of course, 3 hours after noon on a particular day. Noon is here specified as the time 'origin' and the three hours are defined as periods of equal length. They would not need, however, to be defined as of equal length any more than the lengths of the 'block' were, along the street. But if we want, as of course we do, to be able to say that 3 hours on one occasion represent a comparable period of time to 3 hours on another occasion, then we must either specify that the 'hours' are of equal length or, possibly, lay down some rule telling us how the length of the 'hours' is supposed to vary. In the case of the map, you will find that there is quite a simple rule for working out the size of a 'block' at any point in the diagram. (It will be good practice to work this out, if you can.)

You will notice that, whereas to specify the location of the

event, we needed (1) an origin, (2) laws describing the nature of our distance scales (in this case, one law for N–S and E–W but a different law for vertical distances), (3) indications of direction —'N.W.', and (4) three magnitudes; for time we need the origin, a scaling law and only one magnitude, though we also require an indication of '*before* or *after* noon', which we will call 'sign'. This last corresponds to the distinction between 'N.W.' and 'S.E.' in indications of direction.

When we come to the measurement of how hard was the alleged blow, we do not need an origin, but we do require a magnitude and scale of force—each unit of force being defined as equal. It may also be important to specify the direction in which the blow was delivered. This last point is met by describing force as 'a vector' i.e. its direction matters as well as its magnitude.

Most incidents can be described in terms of position, by means of lengths which we write 'L', times 'T', and forces 'F', though we shall see in the next chapter that these do not have to be the fundamental criteria. Indeed we usually prefer to use mass ('M') in place of force. We shall also see later that there are occasions when it is convenient to use all four, L, T, M and F.

Now the confusing thing is that these also are called 'dimensions'. Thus we say not only that 'a map is in two dimensions', or 'a space in three dimensions', but also that 'a velocity has the dimensions of a length divided by a time'—which means that if we drive down Alpha Avenue at a steady speed of 30 m.p.h. (half a mile in a minute), our velocity can be described by dividing half a mile of distance (L) by the one minute of time (T) which we took to cover that distance.

If another car passes us doing 60 m.p.h. its speed is twice ours: it is going twice (2 times) as fast as we are—not 2 miles an hour or 2 velocities, but 2 *times*. The relative speed is then said to be '2' and is called a 'dimensionless number', or a 'numeric'.

If we are slowing down for a traffic light and our speed is changing at a rate of ten feet per second each second, we say that our deceleration (or 'negative acceleration') is '10 ft. per sec. per

sec., or 10 ft. per sec.^{2} and that accelerations so defined must have the dimensions LT^{-2}.

I think that the non-technical reader will now be in a position to follow the more advanced discussion in the subsequent chapters.

CHAPTER III

The Principles of Measurement

WE WILL START AT the beginning—that is 'here and now'. Nothing is more apparent to us than that the material world is extended away from us in space and that events happen in sequence —i.e. 'before' and 'after' in time. For most purposes, the most precise measurement is a spatial measurement and we shall therefore regard spatial measurements as primary. But it must be realized that this is a convention and not even always the best convention. Martin Johnson[26] quotes Milne (1942) as saying that time measurements have a logical priority over distance measurements, since 'to measure length you must say what you mean by two points at the same time and thus in principle you need a definition of distance—simultaneity.* To measure a time, you need only say what you mean by two events at the same place, namely to your individual consciousness.'

Milne's work, however, is directed specifically to problems involving distances and times of astronomical magnitude. Thus for most purposes we can say that two rods are of equal length when, on laying them down alongside one another, their ends coincide, i.e. are to be found 'in the same place' and both ends of the superposed rods are 'here', near enough to be directly observed. We can, indeed we must, also assume that in all ordinary experiments, the rod we are measuring does not change its length when we turn it through a right-angle unless we know of some temperature gradient or other disturbing factor, even though now

*i.e. a simultaneity of two events occurring at different places which cannot be observed at the same time by the same observer—G.W.S.B.

the rod cannot be superposed with the ruler without also rotating the ruler itself.

This is really a matter of definition and we know that there are conditions when it is more convenient not to define length equalities in this way. For example, if a cube is moving very rapidly in the direction of one of its faces, it is convenient to describe its behaviour by saying that its length becomes shorter in the direction of travel than in a direction at right angles to its motion, as measured by an observer who is at rest relative to the motion. But this is a convenience and the facts can equally well be expressed in other ways.[14]*

It is also true that to ask whether astronomical measuring rods change their length on rotation is meaningless. But although to forget these things would be to fall into the very error which I am condemning, namely the division of science into watertight compartments, this book is principally concerned with a particular tract of 'forgotten territory' in which I believe it is simpler to start with lengths and to define time measurements in terms of length measurements.

Dingle claims[14] (p. 29) that length is the only unit which can be measured without recourse to a measurement of any other physical quantity and although I can only agree provided that the word 'measure' has a particular meaning, the point is an important one and should influence our choice of a primary entity for definition.

I think that it would be possible to describe as 'measurement' the determination of equality of weights or even a ratio of weights by judging pitch equality, either directly or in octave, on plucking standard strings to which the weights are attached. For example, we could adjust the weights on a pan until there was no 'beat'. But no one suggests that this is a convenient form of measurement and, with no more ado, I shall accept length measurement as primary for the purposes of this book.

Eddington[18] (p.13) goes so far as to exclude from what he calls

* It is a matter of *definition* that, in the Special Theory of Relativity, the term 'length' is used for $l\sqrt{1-v^2/c^2}$ rather than for l.

'Exact Science' all observations other than the direct counting of objects and the taking of pointer readings, i.e. the fitting of measuring rods to things, the 'exact-scientist' requiring, therefore, no sense organs other than a single colour-blind eye! The blind observer who compares weights by plucking strings and using his ears is thus excluded and indeed much of biology is also eliminated. Eddington himself points out (p. 22) that the selection of the eye as the only sense organ is arbitrary. My dog would certainly protest that the nose would be more reasonable. But the eye has proved most effective for human observers and is the obvious choice if only one sense organ is to be allowed. In Man, the sensory surface of the retina has about a million separate sense organs—far more than in any other sensory apparatus.

S. Augustine[40] (p. 197) fully appreciated the priority of the eye. 'The eyes are the chief of the senses we use for attaining knowledge. . . . Thus we do not say "Hear how it flashes", or "Smell how bright it is" or "Taste how it shines", or "Touch how it glows" because all these things are said to be seen. Yet we do say not only "See how it shines". . . but also "See how it sounds", "See how it smells", "See how it tastes", "See how hot it is". . . . Because though the function of seeing belongs properly to the eyes, yet we apply it to the other senses by analogy when they are in pursuit of the truth about anything.'

Even analytical chemistry would have to exclude some of its best known methods from the field of exact science. When a colourless liquid is run from a burette into a flask containing another colourless liquid, drop by drop, until a blue or a pink tinge marks the completion of the reaction, the colour-blind observer,* though able to read the pointer reading (in this case, the position of the top of the column of liquid on the scale of the

* By 'colour-blind' Eddington clearly means that he cannot perceive colour at all. Ordinary colour blindness would not normally have so drastic an effect. Of course, the change of colour of the liquid could be made to operate an instrument which would record it by the movement of a pointer on a dial. Thus titration as such would not be excluded but only the use of indicators.

burette) would be unable to do the analyses, since he could not observe the end-point.

All this is, of course, a matter of definition, though if it leads to any minimizing of the importance of that part of scientific observation which is not 'exact' in accordance with the definition, or to any further tendency to intellectual snobbery on the part of 'exact scientists', it would be to say the least of it, an unfortunate definition.

That observations should be made with measuring rods does not necessarily imply that the lengths of the measuring rods themselves should be unaffected by their motion or their direction in space, since it is the coincidence of the scales with which we are concerned. We shall see later that there are situations in psycho-physics in which it is convenient to abandon Euclidean geometry but we will not fall into the temptation of using this device too freely. In my own field of Rheology many awkward problems might be solved—or more accurately 'shelved'—by expressing the deformations of bodies in terms of non-Euclidean space but I do not think that this would serve any good purpose.

If we define lengths as equal, then, when they can be laid down next each other and found to coincide, we must define time in terms of length. I shall not discuss problems of simultaneity at a distance but will presume that the order of events located at the observer is directly perceived.

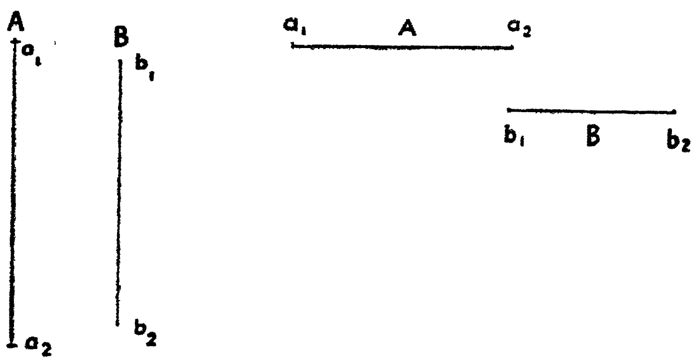
We can only equate time intervals by direct super position when they are already identical, which is pointless and we can only directly define one period of time as greater or less than another when the periods overlap. Consider Campbell's[7]* definition: 'The period [of time] of the system A between the states a_1a_2 is greater than the period of B between b_1b_2 if, when a_1 is not after b_1, a_2 is after b_2.' And for addition of time periods he gives this definition which is only valid when A and B are adjacent: 'The period A + B is that between a_1 and b_2 when a_2 is simultaneous with (i.e. neither before or after) b_1.' Since this reads

* I have corrected what I assume to be a small misprint in the original.

rather like an army Intelligence Test, I have drawn an explanatory diagram, Fig. 2.

In order to define quite independent intervals of time as equal, we must select either a periodic phenomenon like the radiation of an atom or the swing of a pendulum or some type of motion which we *define* as regular.

As Burniston Brown[5] points out, in measuring time by means of a pendulum 'what is observed is the intermittent succession of



Comparison
Fig. 2a

Addition
Fig. 2b

An illustration of Campbell's definitions for comparison and addition of periods of time

alignments between the same pair of marks, one of which is, or has been in motion. The times between successive alignments of the marks are arbitrarily defined to be equal, even if they differ from the impressions of consciousness.'

We shall, of course, be partly directed by the number of events which can intervene during our time interval. It is clear that I can do more things between lunch and tea than I can between reading the words 'lunch' and 'tea' in this sentence. If I spell out the word *T H E* and the word *T* A N N *H* Ä U S *E* R it would be foolish to define the time intervals between the reading of the letters T, H and E in the two cases as equal, since in the latter case I have three other letters to read in the intervals but it would be equally

rash to assume that on different occasions I shall always spell out the words at the same rate. This can only be fixed by defining a 'clock'; but before discussing clocks there is one further and perhaps very obvious point to be noticed. Whereas there can be no doubt that in reading aloud, not only is H between T and E, but the direction of the sounds is fixed—the H is 'after' the T and 'before' the E—in print, this is purely conventional since in many languages words are read from right to left.

With regard to the choice of a clock, Poincaré[34] long ago pointed out that there is no absolute clock and that the best is the one which most simply expresses the equations of mechanics.

Campbell[7] says that it is very difficult to choose such 'a repetitive iso-periodic system' as he calls it, since the entire state of the universe cannot be restored and all concepts of 'period' are themselves associated with change.

As Dingle[18] has pointed out, the definition of times as equal when light in free space or alternatively a Newtonian body, free from the influence of other matter, takes such times to traverse superposably equal distances, has many practical advantages. 'Newtonian mechanics', he says, 'is peculiarly fitted for the treatment of light as a periodic phenomenon because the association of Time and Space measurements implied in Newton's first law is just that which makes wave-length and frequency change by reciprocal factors, thus keeping the velocity constant.'

Milne claims that the frequency clock does not tell precisely the same time as the velocity clock but the effects of this are only apparent over periods of time comparable with the age of the universe since one of these clocks would have appeared to have 'started' some 2×10^9 years ago in terms of the eternal constancy of the other.

For our purpose we shall not worry about this, but it is worth remembering that when it was found that the rotation of the earth was gradually slowing down when measured by such a Newtonian clock, Newton's laws were taken, quite arbitrarily, as correct and the earth was regarded as 'going slow'. This is all very natural if we follow Poincaré's criterion of simplicity of mechanical laws;

nor, I think, need we be too much upset by the problems introduced by General Relativity about the meaning of equality of time intervals in gravitational fields.

We must, however, be careful when we come to apply Poincaré's criterion to biological and psycho-physical processes; and even, as I have shown,[38] to the behaviour of complex non-living systems like some of the plastics.

Poincaré is specifically referring to astronomy—whether we should extend his criterion in the case of other sciences to read 'the equations describing the phenomena of interest to us' is a point requiring further consideration. In early times,* somatic phenomena, like pulse and breathing rates, were used as time-scales just as body lengths ('feet' etc.) were the original units of length.

There can be no question but that, so far as the 'laws of mechanics' are concerned, the natural time scale for normal use is Newtonian. But smaller organisms would appear also to have their own natural time-scales. One might even suggest that these time-scales are specifically characteristic of independent organization, though not, of course, necessarily of living organisms.

This point was made as foreshadowed in 1933 by Bp E. W. Barnes[2] when he wrote: 'It is probable that an even more far-reaching revision of Newtonian concepts will be made when the full consequences are seen of the knowledge that the physical Universe is a linked system in which no bodies are, in the Newtonian sense, independent.'

In so far as time is concerned, we may, for convenience, consider three kinds of system without implying hard and fast limits between them.

First there are dynamical systems which are 'reversible' in the sense that they return spontaneously to states indistinguishable from their previous states. Good approximations are planets and frictionless clocks of all kinds. These systems form the basis of time measurement, since we can count the number of their cycles, but they do not show the direction of the arrow of time.

* Mach[31] tells us that Galileo used these time scales.

Secondly, we have statistical systems. These show irreversible changes when isolated, but may be returned to their original state if energy is supplied from outside. They indicate the directional nature of time but do not in themselves provide a measure of time.

Thirdly, there are what we may call vital systems, though I would not imply that they include only living matter. Such systems cannot be returned to a previous state even with outside help and can never serve as measures of Newtonian time but they indicate very clearly its directional character.

It is this last type of behaviour, characteristic of 'vital systems', which is associated with what Bergson called 'la durée'.

Complex inanimate materials like some of the plastics are 'statistical', but, in so far as our experiments are concerned, it is *impracticable* to return them to their initial state. They thus link up very closely with vital systems and this is why, if mathematical simplicity were our sole objective, we should not use a Newtonian time-scale to describe their behaviour.

Du Noüy[32] has made a careful study of the rate of healing of sterile wounds in men and animals. He finds that the rate of healing is a function of the area of the wound and the age of the animal. Except in pathological cases, notably diabetes, he found that there is very little fluctuation from this simple functional relation. The duration of life of cells *in vitro* shows a similar relation to the age of the animal at the time when the cells were removed. As man grows older, his physiological clock slows down in terms of Newtonian clocks and it therefore seems to him that time passes more quickly. Du Noüy suggests a simple logarithmic biological time scale.

It would be rash to suppose, however, that our psychological time scale reflects no more than the discrepancies between the physiological and Newtonian scales. Our physiological processes do not necessarily slow down when we are apprehensive or dull and when time hangs heavy on our hands!

Yet, though we measure time by what are called 'dynamical systems', i.e. 'clocks' which must be as nearly as possible unin-

fluenced by changes in temperature, the changing processes with which time is associated in its unidirectional aspects are highly temperature-dependent. Indeed some go so far as to associate the unidirectional character of time entirely with the process of temperature equalization. I believe this to be incorrect. But it is not surprising that both physiological and psychological time scales are influenced by temperature changes.

In ordinary mechanical clocks, which we try to make as independent of temperature changes as is possible, there is also a loss of energy by friction which we attempt to reduce to a minimum. This means that atoms or molecules of the surrounding matter which are not part of the clock collect some of the energy from the clock itself and this energy has to be replaced by means of a spring wound by hand from time to time or by an electric current or in other similar ways. The best clocks have the minimum friction and the minimum temperature dependence.

Perhaps the most important characteristic of living matter is its capacity to slow down the process of the 'levelling out' of energy. As Schrödinger[37] points out: 'The device by which an organism maintains itself stationary at a fairly high level of orderliness (=fairly low level of entropy*) really consists in continually sucking orderliness from its environment. . . . In the case of higher animals we know the kind of orderliness they feed upon well enough, viz. the extremely well-ordered state of matter in more or less complicated organic compounds, which serve them as foodstuffs.'

On the physiological side, it has been shown that a rise in temperature causes bees, ants and termites to reduce their customary interval between meals and Hoagland[24] made a feverish patient (his wife) count at a speed which she believed to be seconds and found a logarithmic relationship between the rate of counting and the reciprocal of body temperature (°K). She was, in fact, comparing time intervals during her illness with stored

* 'Entropy' is a technical word meaning the degree of disorder or 'rundownness', the point being that ordered systems with their energy unevenly distributed tend to break down to lower levels of 'orderliness'.

unconscious memories of time intervals previously established in relation to a Newtonian clock.

Hoagland later collected further data from subjects who were given artificial fever by diathermy and he also found some data published by François on the frequency of tapping of a prescribed rhythm at different body temperatures. Hoagland suggests that our psychological 'clock' may be controlled by chemical processes (oxidations) in the brain. The rate of such processes would be most simply related to temperatures expressed on the Absolute

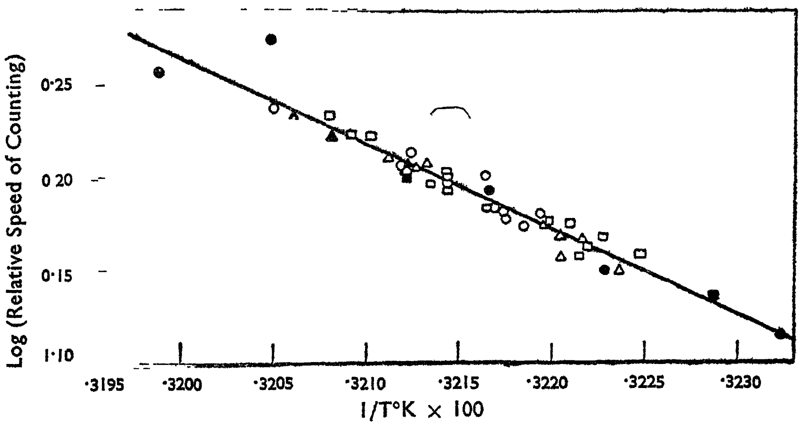


Fig 3

Temperature Characteristics of Human Time Sense

From H. Hoagland, J.Gen.Psychol. 9 267 (1933)

Scale (°K)—i.e. in relation to absolute zero of temperature. Unfortunately even so wide a change in body temperature as that from 97°F. to 103°F. corresponds to no more than 3.3°K on the Absolute Scale, i.e. a very small proportion (1.2%) of the temperature range from absolute zero. But the data, reproduced, with permission, in Fig. 3, from Hoagland's paper show a remarkable regularity.

The reader is probably aware that the electroencephalograph is an instrument which measures fluctuating brain potentials which can be picked up by electrodes attached to the scalp. These represent fluctuations in electrical activity within the brain cortex

itself. The 'shape' of the waves is complex but, under resting conditions, there is a well-marked rhythm at a frequency of about ten per second which is known as the 'α-wave'. Hoagland has shown that the frequency of these α-waves from the electro-encephalogram are likewise dependent on body temperature.

Piéron[33] suggests that in internment camps where bad nutrition and general debility may lower body temperature by at least $1\frac{1}{2}$°F. there should be a marked speeding up of sense of duration—but, as he points out, this is likely to be more than compensated for by purely psychological factors! He also claims that with bees, the acceleration of time is a linear function of the logarithm of absolute temperature, with the same proportionality constant as that shown by man. But with bees, a wider range of temperature is possible. For an increase of 10°, the unit of time is said to be reduced to a third of its normal value.

Bergson[3]* has given a wider definition of equality in time units: 'Two intervals of time are equal when two identical bodies, in identical conditions at the beginning of each of these intervals and subject to the same actions and influences of every kind, have traversed the same space at the end of these intervals.' According to Bergson's own philosophy, living matter could never qualify to be a 'clock' in this sense.

Where does all this lead us? I think that it leads us to a new concept of 'Process'. In the purely mechanical field, we generally think of process in terms of velocities or accelerations. Thus the process of the transmission of energy is described in terms of an experimentally measured 'constant velocity of light'; bodies fall in a gravitational field with a 'constant acceleration' (i.e. constant increase of velocity per second).

Why is the speed of light or of bodies freed from the influence of other bodies, constant? *Because we define our equality of intervals of time in such a way as to necessitate their constancy.* The *experimental* fact is that there are quite a number of apparently independent 'clocks' which tell exactly or almost exactly the same time:

* Pogson's translation, p. 115 (1910).

the natural radiation frequency of atoms, the motion of light and of rigid bodies uninfluenced by gravitational fields, etc

When we come to consider force, we find much the same situation. We certainly have a direct perception of force but it is clear that in Newton's law it is *defined* as the rate at which a body changes its momentum. (Whittaker[48] tells us that Leibnitz rejected the whole idea of such a definition.)

I remember when I first learned about Newton's laws, being very much puzzled about this. I was told that to convert a mass into units of force, one must multiply by the acceleration of gravity, 'g'. This is the acceleration with which the body would fall in a vacuum. 'Yes,' I said, 'but the body *isn't* falling in a vacuum and isn't going to fall anywhere, I hope. It's a part of my apparatus—what has 'g' got to do with it?' Perhaps if we had been taught to regard force as either kinetic or potential as we had learned to think of energy, I should have found it easier to understand.

Here again, we meet an experimental dimensional constant 'G', which relates the gravitational force between two bodies to the product of their masses and the square of the distance between them. We shall see in the next chapter that these dimensional constants can often be eliminated if we care to reduce the number of our primary quantities.

Equality of forces does not depend on the constancy of the speed of light though I think it is impossible to find a definition of equality (without introducing accelerations and hence Newtonian time) that does not depend on a constancy of direction which is in fact the path of a light-ray. I am not sure whether a straight line, in the absence of a gravitational field, should be *defined* in terms of the path of a light-ray, as has been done by Blamey Stevens[42]. Mach[31] long ago denied this possibility when he wrote: 'We can derive the *metrical properties* of straight lines from rays of light just as little as we can derive them from *imagined* straight lines. For this purpose, experiences with *physical* objects are absolutely necessary' (italics in original translation). It is true to say, however, that, just as we *define* the velocity of a

body uninfluenced by other matter as constant by accepting such a body as our clock, so we *define* its path as a straight line by specifying that there are no forces acting on it. Moreover, forces are equal if, when acting on a body in opposite directions *in a straight line*, they do not change its motion.

We see then, that we select a time scale and define our forces in such a way that simple dynamical processes, least influenced by temperature changes, will lead to constant velocities and accelerations. We find, of course, that for comparatively simple physical systems, even systems where the particles are essentially dynamical but where the whole assembly is highly temperature-dependent such as gases, other quite simple groupings of (normally) lengths, masses and times are found to be constant. This leads us to the concept of 'physical properties' which we shall consider in a later chapter.

CHAPTER IV

The Physical Theory of Dimensions

I DO NOT INTEND to repeat in this chapter the classical treatment of the theory of dimensions except in so far as would seem to be necessary to make clear to the non-technical reader what the problem is about. For those who want to study the theory more fully, I should recommend books by N. R. Campbell[7] and P. W. Bridgman[4] for a general discussion, and A. W. Porter's little monograph[35] for the practical applications.

The principle involved is that one cannot make direct comparisons of magnitude between things which are qualitatively unlike. In its simplest form, it seems fairly obvious that there is no clear meaning to be attached to such an equation as:

$$48 \text{ inches} + 3 \text{ hours} = 4 \text{ feet} + 180 \text{ minutes}^*$$

Such an equation is said to be 'dimensionally inhomogeneous' even though 'true'. The equation is, of course, only ridiculous so long as we regard times and lengths as different in kind. I should certainly regard as homogeneous the equation:

$$3 \text{ potatoes} + 6 \text{ onions} = 9 \text{ vegetables}$$

All measurements of entities of this kind require (1) a scale which must be defined, (2) a definition of equality of units, and (3) a number which gives the relationship between the scale unit and whatever is being measured. In a sense all measurements are expressed in dimensionless form. 'Three inches' means three *times*

* Professor Dingle has published a series of interesting and provocative papers in the *Philosophical Magazine* (1942–1949) which have given rise to considerable controversy. This equation is quoted from one of his papers where he gives it as an illustration of dimensional inhomogeneity.

as long as the standard inch; and four seconds, four *times* as long as the standard second, but this does not mean, of course, that inches and seconds are dimensionally compatible. They can only be regarded as directly comparable if, for example, we regard one second as equivalent to the distance travelled by light in that time and convert our inches, hours, feet and minutes into the same units of length. The equation could be written:

$$48 \text{ inches} + 13.\ 10^{13} \text{ inches} = 48 \text{ inches} + 13.\ 10^{13} \text{ inches},$$

which is dimensionally homogeneous and simply expresses an identity. This would mean using a time as if it were an alternative way of expressing a length, the velocity of light being not a 'velocity' in the ordinary sense at all but a universal constant which it is convenient to regard as dimensionless. This type of treatment has been developed by many authors. Stevens[42] bases his 'Identity Theory' on it and Burniston Brown[5] has proposed to eliminate Mass as a dimensional primary quantity by regarding 'G' as dimensionless. Thus Force = Mass × Acceleration and *gravitational* force is also proportional to $(\text{Mass})^2 \div (\text{Distance})^2$, hence Mass will have the dimensions of an Acceleration × $(\text{Length})^2$, if the proportionality constant is regarded as dimensionless. But we must remember that there are forces which are not gravitational.

We saw in Chapter II that when we derive a velocity from measurements of length and time, we say that it 'has the dimensions L/T'. Likewise an acceleration 'has the dimensions L/T^2'. A force is defined as the product of a mass and an acceleration and therefore 'has the dimensions MLT^{-2}'. We ascribe to physical quantities, then, dimensions in a number of primary quantities (usually three) depending on the way in which their measurement is generally to be made.

Most electrical quantities, as usually defined, turn out to have fractional dimensions, e.g. for electric charge, $M^{1/2}L^{3/2}T^{-1}$*.

* Since force $= \dfrac{(\text{charge})^2}{(\text{distance})^2} \times$ dielectric constant of empty space and this constant is assumed to be unity.

There is no valid theoretical objection to fractional indices, of course, but it is somewhat 'tidier' to avoid them. As Dingle says: 'the root of the objections to fractional indices is again the idea that dimensions characterize physical quantities instead of the arbitrarily adopted method of measuring them.' He goes on to point out that the dimensions of an acceleration are normally given as LT^{-2}; i.e. a length divided by a 'square second' and that a 'square second' *per se* would be just as meaningless if it were meant to characterize a physical quantity, as is the square root of a mass ($M^{\frac{1}{2}}$).

Burniston Brown's treatment has not been accepted without criticism and I quote it here purely as an illustration of the type of transformation that can be made.

Inhomogeneous equations need not be altogether meaningless and, as an illustration of this, Bridgman[4] (p. 42) quotes an equation obtained by adding the velocity (V) and acceleration (g) equations for a falling body:

$$V+s=g\,t+\tfrac{1}{2}\,g\,t^2 \text{ (where s is distance and t is time)}$$

Such an equation is 'inhomogeneous' but 'complete'.

What are we to feel about expressing times and masses as equivalent to lengths? Do we commit ourselves to such errors as that of saying that time 'is only another kind of space'?

I think not. Whittaker[48] (p. 58) in his admirable little book *Space and Spirit* points out the essential distinction between formalism and realism in such cases. He is discussing the historical development of the idea that the earth is not the centre of the Universe and the remarkable fact that it was 'not until 1616—seventy-three years after the death of Copernicus—that the affair of Galileo began, and the Congregation of the Index found his teaching in certain respects objectionable'.

Indeed *De Revolutionibus* had been dedicated to a Pope and was prefaced by a letter from a Cardinal! Whittaker continues—'The amount of opposition which a new doctrine like that of Copernicus might be expected to evoke depended very much on whether it offered itself merely as an improved way of "saving

appearances", or whether it claimed to provide a new understanding of the nature of things. In the former case . . . it would be merely an affair of the mathematicians . . . but in the latter case . . . it might give rise to heated controversy.'

There is no profound philosophy involved in eliminating universal constants. It is purely a matter of convenience. Campbell very sensibly recommends, however, that no constant should be eliminated until it is adequately understood.

Dingle and Bridgman have both emphasized the point that the choice of dimensions is arbitrary. For example, to quote an illustration given by the former, if we are moving towards or away from a source of light or sound, the colour or pitch appears to change—we often notice the way in which the pitch of a whistle or a bell seems to drop as we pass in a fast-moving train. It is easy to calculate our speed from the ratio of change in wave-length to normal wave-length, which is, of course, a dimensionless number and Dingle points out that we should be quite justified in *defining* velocity by this number instead of as a length divided by a time. Or as Bridgman has put it: 'There is nothing absolute about dimensions . . . they may be anything consistent with a set of definitions which agree with the experimental facts.'

A case about which there has been much discussion is that of temperature. This is sometimes taken as an independent primary quantity (Θ) so that such entities as specific heat and entropy will have dimension including Θ, viz. $L^2T^{-2}\Theta^{-1}$ and $ML^2T^{-2}\Theta^{-1}$ respectively. The situation becomes very much simplified however if, in the equations relating the pressure of a gas (p) to its specific volume (volume per unit mass v)* and absolute temperature (T): $pv=RT$, we define the constant R as a dimensionless number. This gives T the dimensions $[ML^{-1}T^{-2}]\times[M^{-1}L^3]=L^2T^{-2}$ (an energy per unit mass) which leads to much simpler dimensions for specific heat (dimensionless) and entropy (M). But I think Porter[35] goes too far in saying that this procedure 'reveals the real dimensions of Θ'. Rather does it give us a more convenient and

* A number of authors have regarded v simply as a volume, which gives temperature the dimension of energy, ML^2T^{-2}.

effective way of expressing its dimensions and also those of a number of other entities involving temperature.

On the other hand, there is a sense in which one aspect of a phenomenon is more 'real' than others even when the choice of aspects is within our power. As I was listening, recently, to a lovely rendering of a Bach Concerto, it occurred to me that even a man born deaf could have some slight conception of the beautiful symmetry of the work if he were shown a harmonic analysis of the music. The wave-forms showing the variations in pitch and intensity of sound would in themselves have a certain beauty. (See p. 68). But it would be absurd to suggest that they could give to a man who had never heard, any conception of the 'real' meaning of the music, though they could be made fully to describe the physical phenomena produced by the performers. I think that this presents certain analogies to the problem of the choice of fundamental dimensional units.

It is not as a rule convenient to work with either a very small or a very large number of primary quantities. As a rule Mass, Length and Time are convenient, though, apart from the question of the number of quantities, it is sometimes an advantage to use Force in place of Mass. Dunne[16] points out that this formulation leads to very simple dimensional expressions for Force (F), Momentum (FT), Energy (FL) and Action (FLT). In some branches of physics, such as rheology,* I think that it would generally be preferable to use this system in place of the usual MLT, when there are no accelerations, but such changes are always difficult to make.

It is well known that it is often possible to find the form of complex physical laws simply by applying the principle of dimensional homogeneity. In some cases, indeed, the problem is too complex to be solved in any other way. This method does not give us the numerical factors, of course, but only the dimensions of the primary quantities.

In rheology, for example, we often measure the viscosity of fluids by timing the fall of a small metal ball over a given distance

* The study of the deformation and flow of matter.

through the fluid. It is apparent that, under the conditions of the test, the rate of fall (V) will depend on the radius of the ball (r), the density difference between ball and fluid ($\Delta\rho$), the acceleration produced by gravity (g) and the viscosity of the fluid (η). Stokes has made the full calculation of the relation between these things, but even with the necessary simplifying assumptions, it is by no means easy to follow. We can write an equation:

$$V \propto (r^a . \Delta\rho^b . g^c . \eta^d)$$

which simply means that the velocity of fall of the ball is a function of unknown powers of radius, density difference, g and viscosity.

Now we know that using the ordinary dimensions of Mass, Length and Time, the powers to which Mass is raised for r, $\Delta\rho$ g and η must add up to zero, since velocity does not include Mass. Likewise since velocity is given by L/T, the lengths must add up to +1 and the times to −1. This gives us three equations but there are four unknown quantities—so we cannot know the values a, b, c, and d independently unless we make a postulate that two of them are likely to be equal—for example that $\Delta\rho$ and g are likely to have the same exponent since the gravitational force driving the ball will be proportional to the simple product of $\Delta\rho$ and g.

If, however, we use four fundamental magnitudes, M, L, T, and F, and describe η and g in terms of Force, we have four equations and hence we can solve for a, b, c, and d independently. (This very simple calculation is given in Appendix I.) What we are really doing, of course, is to define force as proportional and not equal to mass×acceleration, the constant of proportionality having dimensions $F\,M^{-1}\,L\,T^2$. It must be realized, however, that this is *purely a matter of formulation.* We are not getting something for nothing and we are making just as much of an assumption in postulating a universal dimensional constant to relate force to mass as we are in writing b=c in the former method. The relationships between the numbers of fundamental magnitudes, dimensional constants, etc., are formulated in a

theorem known as the 'Π—theorem' which is fully discussed by Bridgman[4].

There are limitations, of course, to what sort of magnitudes we may regard as 'primary'. To quote Bridgman: 'Force is perfectly well adapted to be used as a primary quantity since we know what we mean by saying that one force is twice another and the physical processes are known by which force may be measured in terms of units of its own kind.' The same would be true of velocity. We shall discuss this point more fully in the next chapter.

Physicists generally like to find dimensionless groups of properties to describe phenomena and indeed this is exceedingly useful. For example, Stokes' equation, which we have already discussed, only holds so long as the ball is not falling fast enough to produce appreciable turbulence in the liquid. We more frequently meet this problem of turbulence when liquids are streaming through pipes and the like, and it has been found that turbulence starts when a certain 'number'—the so-called 'Reynolds Number', i.e. a dimensionless group of properties, reaches a definite critical value. By a 'number' we mean in this sense, that the figure given to represent this quantity does not depend on any particular choice of units. We have a different figure to express a measure of time depending on whether we use minutes or hours and likewise with weights, lengths, velocities and so on. Thus a speed of 10 miles an hour is the same as one of 14.7 feet per second. But we have seen that velocity could also be described as a ratio of wave-lengths. If the wave-length of a particular monochromatic (single wave-lengthed) light appeared to be increased by 1% as a result of our moving away from the light-source, it makes no difference whether the lengths themselves are measured in fractions of an inch or of a centimetre, the velocity would still be given by

$$\frac{\text{increase in wave-length}}{\text{normal wave-length}} = \frac{1}{100}$$

provided only that both wave-lengths were expressed in the same units. The Reynolds Number, defined as diameter × velocity ×

density÷viscosity, is the same in any given case whether lengths are given in centimetres or inches, weights in grams or pounds, and time in seconds or hours, so long as the same system is used throughout. Thus, using centimetres, grams and seconds, we have:

$$\frac{\text{diameter (L)} \times \text{velocity } (L\,T^{-1}) \times \text{density } (ML^{-3})}{\text{viscosity } (=\text{pressure} \div \text{rate of flow or } ML^{-1}T^{-1})} = \text{a dimensionless number.}$$

The Law of Dimensional Homogeneity does not prevent our adding together any dimensionless quantities but such a process is by no means always meaningful. For example, as ordinarily defined, angles are dimensionless—but we could seldom attach any meaning to the addition of an angle to a Reynolds Number. Likewise, with dimensional properties, according to the usual definition, a thermal conductivity has the same dimensions as a viscosity but we do not add such properties to one another or compare them directly in magnitude.

It is, of course, always within our power to ensure dimensional homogeneity in an equation by introducing a suitable dimensional constant—not a universal constant, but one varying for each specific case.

For example, take the case of a very simple equation:

$$y = A\,x^n$$

If y is a length and x a mass, then if n is 1, A must have the dimensions L/M or if n is O, of L. If n is a parameter, i.e. constant for any one set of circumstances but differing from one material or set of conditions to another, then A will have to have different dimensions for each material or set of conditions. In such cases, A is not as satisfactory a measure of general behaviour as it would be if its dimensions were always the same; but it is definitely incorrect to describe the equation as 'inhomogeneous' provided that the dimensions of A are correctly adjusted. We must be careful, of course, what use we make of A and how we describe it. There is no reason to deny to A the title of a physical quantity. Both Dingle and Burniston Brown agree that a physical quantity is anything that can be measured (Dingle adds 'by one or more

strictly definable processes')*. But knowing the values of A for two materials will not by itself give us a direct means of comparing their behaviour unless we are also given the value of n. We will return to this question in a later chapter.

In such cases, physicists generally prefer to try to find some characteristic values of x and y (we will call them x_0 and y_0) which are themselves properties of the materials so that the equation can be written

$$\frac{y}{y_0} = A\left(\frac{x}{x_0}\right)^n \text{ which is, of course, dimensionless.}$$

This is excellent when it can be done naturally—i.e. when x_0 and y_0 really are characteristic of the material but there seems to be no advantage in introducing such constants unless they can be shown to be meaningful.

In the first of the two equations the dimensional status of A presents some interesting features which have been specifically studied by Dingle. He regards the use of such equations as representing 'an enlargement of the scope of the hitherto contemplated physical relations' and that 'no objection lies against it on dimensional grounds'.

But to understand this question properly, we must first examine the relationships which exist between measurements and physical properties.

* Later, Dingle prefers to avoid the use of such terms altogether.

CHAPTER V

Dimensions in Relation to 'Properties' and with Reference to Colour-Matching

It is possible that, on reading the last two chapters, those who are not very familiar with modern trends in physics will be surprised that the principles of physical measurement and the allocation of dimensions to physical quantities should now be regarded as so much less rigid than was at one time generally supposed.

Indeed the earlier classifications of types of measurement and of physical magnitudes were extremely complex and it is doubtful in view of the general fluidity of the situation if the complexity was justified.

For example, Campbell[7] defined two kinds of measurement, fundamental and derived, which we may abbreviate as F and D. A measurement of length is a fundamental measurement, being undertaken as a single operation: that of density when performed by weighing and measuring a volume with subsequent division, is a derived measurement. He also defined A and B magnitudes (the former can be added whereas the latter cannot) and for any given process of measurement, primary (P) and secondary (S) magnitudes. When density is measured by weighing a known volume of a liquid the weight is the primary magnitude: when the volume of a mass of known density is measured it is the volume which is primary. This would seem to lead to all possible combinations of F or D, A or B, P or S. In fact most of the combinations can be shown to occur. Campbell claims that the only combinations which do not occur are BPF and BSF.

But it is clear from the earlier chapters that it is within our

power to change the number of fundamental magnitudes* so that so complex a system seems unnecessary. Bridgman and Dingle have independently developed rather similar but simpler classifications, the former some years before Campbell's book was published, and the latter considerably later. I am indebted to the writings of all three authors for much of the first half of this chapter.

Bridgman speaks of 'primary and secondary quantities', the primary quantities being the arbitrarily selected units of dimensions (normally Mass, Length and Time) as discussed in the last chapter. Secondary quantities, like velocity as usually defined, are built up from measurements of primary quantities. We have already seen that there is no reason why such quantities as velocity and force should not be taken as primary but Bridgman points out that we should not take as primaries, quantities which cannot be compared directly.

All this leads us to the question of the fundamental nature of physical properties. Physical quantities or properties used to be thought of, and are still thought of by some physicists, as characterized by specific dimensional symbols which are independent of the processes by which they are measured. But it will surely be clear to the reader that a dimensional symbol must in truth describe not a property in this sense, but a process of measurement. Indeed in his latest work, Dingle denies all significance to such expressions as 'physical quantity' or 'property' and regards physical magnitudes simply as the result of specific processes of measurement.

We have already seen something of the reason for this when we were discussing the invariance of length of measuring rods attached to rapidly moving bodies. To recapitulate† by a quotation from Dingle's first paper on dimensions (1942) 'Measurements of a length by a metre rod and by triangulation may be different, in which case, if we wish to use both methods, we must

* Campbell himself is quite clear about this.

† I make no apology for seeming to harp on this point: it is of vital importance. For very similar views in relation to mental measurement *see* Burt[6] p. 112.

state which has final authority. If we choose the metre rod, then we say that space is non-Euclidean; if we choose triangulation, we say the metre rod has expanded or contracted. To get unambiguous results it is clear that for every physical quantity we must choose a particular process of measurement which we regard as canonical.'

A striking illustration of the need for canonical definitions is found in the case of temperature, whose dimensions we have discussed in the last chapter and the measurement of which is dealt with in a 'Discussion on Units and Standards' held by the Royal Society and published in the Proceedings in 1946. J. A. Hall reported on the definitions of temperature which vary over different ranges. Thus from −183° to 660° C, temperature is defined in terms of the resistance of a platinum wire; from 660° to 1063° by the electro-magnetic force of a standard thermocouple and above this last temperature by the intensity of radiation of a defined wave-length from a 'black body'. We need not worry about the physical processes involved in these measurements but particular interest lies in the fact that these conventions result in serious ambiguity, since it so happens that pure aluminium freezes ('becomes solid') at just over 660° C using the resistance thermometer, thus falling within the official range of the thermocouple, whereas when the thermocouple is used, the freezing temperature is just below 660° implying that the resistance thermometer is required! Until a suitable adjustment was made, it was therefore impossible to quote an accuracy freezing point for aluminium.

Temperature is a B-magnitude and, as such, its dependence on the method of measurement is undisputed. But even with A-magnitudes, which can be measured directly and which can be added together, Professor Dingle has stressed the independence of different methods of measurement.

I want to make it quite clear that this is not merely a matter of carelessness of definition. It is inherent in the nature of things that these concepts which we call physical properties or quantities are abstractions from specific physical situations. Science is possible not because of their invariance but because of the comparatively

small magnitude of the adjustments which have to be made in order to relate the different processes.

We have seen that this is true with measurements of time—perfect Newtonian systems are no more than imaginary prototypes. Please do not think for a moment that I am criticizing our use of them any more than that I would abandon the Newtonian time scale. But we must remember that our whole way of thinking on these matters is greatly influenced by the Greek philosophers and especially by Plato. The habit of referring the imperfect systems of the real world to imaginary perfect 'forms' is so useful in science that one could hardly exaggerate its importance, were it not for the fact that there is to-day an undoubted danger that the physicist may forget that he is only one stage nearer 'perfection' than his colleagues who practice less exact branches of science. The difference is one of degree and not of kind.*

We will pass, then, from the field of pure physics, with its observer with one colour-blind eye, without so much as opening a new chapter, to consider the interesting problem of matching colours which presents many parallels to the problems of pure physics. I recommend to the reader a good popular account of this question by G. Herdan[23]. Herdan regards 'the universe of colour as a manifold distinct from the rest of physical phenomena' but, be that as it may,† there can be no doubt that a separate set of dimensional units is required for its description. Herdan describes brightness, chromaticity and saturation as the three 'attributes' of colour but, although they are not entirely independent of one another, I think it would not be unreasonable to refer to them as 'dimensions'. We can prepare a set of panels ranging from black, through various shades of grey, to white, which differ, therefore, only in *brightness*. A set of panels of the same brightness but differing in hue will vary only in *chromaticity* as it is called,

* This aspect of reality has been much emphasized by the French Existentialist philosophers.

† R. Donaldson[15] has made an interesting attempt to link visual and photometric methods of colour matching but this does not invalidate Herdan's point. See also *Researches on Normal and Defective Colour Vision*, by W. D. Wright.[50]

and finally, *saturation* is defined as the departure of a chromatic panel from the equivalent grey.

These three 'dimensions' can be depicted by means of a 'colour solid' which is shown diagrammatically in the figure 4 taken by permission from Herdan's article.

(The question of nomenclature is highly complex and controversial. The reader should consult the Physical Society's

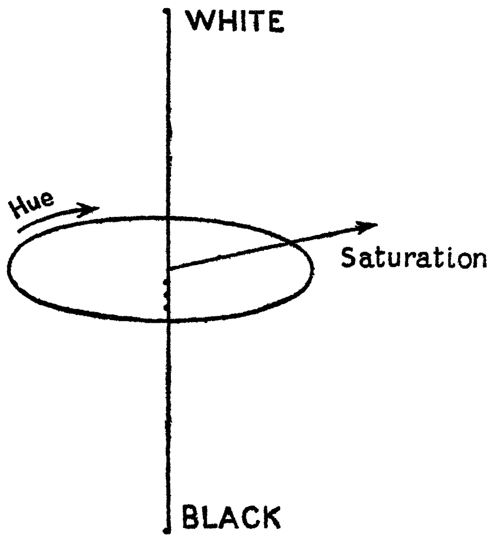


Fig. 4
Scheme of colour solid
From G. Herdan, Brit. Sci. News 1, No. 4, p. 12, 1947

Colour Group Committee Report on Colour Terminology, 1948.)

The position of a colour in such a model requires three magnitudes for its description. The height indicates its degree of approximation to whiteness or blackness and is measured by its proximity as intermediate between the extremes of white and black (we shall refer to this point in a later chapter). The distance from the vertical axis defines the extent to which the sample is coloured, from a colourless grey at the centre, to full saturation

at the periphery. The position round the circle indicates the hue. These are then three nearly independent magnitudes.

It is pointed out, however, that the usefulness of the colour solid is limited by the fact that it is actually used by building up a series of individual coloured panels each varying slightly in the required dimensions from its neighbours, with which the comparison is made. 'It is a pseudo-quantitative sort of representation, since the spacing of the panels in any direction is only a sort of ranking and their distance from the origin or the zero-point of any co-ordinate correspond only to the ordinal numbers.'*

It is well known that most colours (hues) can be made by mixing the three primary colours in suitable proportions. In a few cases it would be necessary to add one of the primary colours first, i.e. one of the components would be said to be present in a negative quantity. Wright,[50] for example, (p. 122) says 'No additive combination of red, green and blue produces an adequate match for a spectral blue-green'. Herdan specifically describes the manifold of colour as 'three dimensional' in this sense. These dimensions correspond to the three dimensions of Euclidean space, and this analogy does not interfere with our previous comparison of brightness, hue and saturation with mass, length and time.

The perception of chromaticity in terms of three components is comparable with our intuitive perception of space as three dimensional: both would seem to be physiological processes. In some types of colour blindness, for example, only two-colour matching is possible.

Mach[81] long ago (1902) discussed the analogy between our three-dimensional perception of space and the (normally) threefold aspect of colour. He points out that 'Whereas nearly equal distances in sensuous space are immediately recognized as such, a like remark cannot be made of differences of colours, and in this latter province, it is not possible to compare physiologically the different portions with one another.' Mach has an interesting

* Compare this with the concept of 'distensive magnitude' discussed later (p. 63).

suggestion to make as to why we perceive space specifically in three dimensions: he points out that man and most animals 'have three cardinal directions distinctly marked on their bodies'. Up differs from down, left from right, and front from back but not as a rule as widely as do up from left or left from front.

The physiology of colour vision is still a highly controversial issue and certainly one which I am not competent to discuss* but empirical evidence for trichromaticity allows quite simple measurements of colour. For example, J. Guild, (whom we shall refer to again in the next chapter) and others have designed very accurate modern colorimeters in which half the visual field is occupied by light from the colour to be matched and the other half by mixing colours until the match is achieved. A complex colour can thus be described in terms of x units of red, y of green and z of blue. The magnitude x, y and z can be plotted on a triangular graph, or, in more recent times in the form of rather more complicated graphs.

Burt[6] (p. 84) has an interesting comment on this question: 'It is easy to show that these equations [the colour equations obtained from quantitative experiments on the effects of mixing lights of various hues] are reducible to terms of three independent variables only—the so-called primary colours, these colour factors can be represented by three dimensions of the familiar double colour pyramid. We may even determine what three primary colours will best fit the actual data. But whether there are really three separate retinal substances or processes corresponding precisely to these theoretical primaries is a question that cannot be decided from the colour equations alone.'

Herdan goes on to point out that equal distances between points in different parts of the colour chart do not imply equal sense differences, or conversely, those differences in hue which the eye judges as equal do not correspond to equal distances on the colour chart. In order to obviate this difficulty, a mathematical

* See Prof. W. E. Le Gros Clark's fifty-ninth Robert Boyle lecture, 1947. Pickford, as a result of factorial analysis (see Chapter VI) favours theories postulating at least four primary colours.

expression is derived which, for small distances, will remain 'invariant' on the chart—that is, independent of the co-ordinates.

This is very similar to the use of Gaussian co-ordinates in the General Theory of Relativity. We may regard colour-space as 'curved', and we assume that the curvature may be ignored only within very small elements of the space. The curvature can be expressed by means of six numbers (for a three-dimensional solid diagram such as we are considering). These numbers are written as g_{11}, g_{22}, g_{33}, g_{12}, g_{23}, and g_{13}. This must not, of course, be confused with the 'g' we have already met—the acceleration of gravity, nor yet with Spearman's 'g', the factor of general mental ability. It is a pity that there are not enough letters of the alphabet to prevent such risks of ambiguity. These uses of 'g' are all so well established that I dare not ignore them.

Any further discussion on the meaning of the g's which measure curvature must involve a little mathematics and it has therefore been relegated to Appendix II.

To interpret this procedure in terms of colour-space, we can surround the position on the diagram of any given colour by an ellipsoid (i.e. an oval figure) whose orientation and shape will represent the uncertainty of matching colour at that particular point on the diagram; the points on the surface of the ellipsoid being placed at distances from the centre which correspond to just noticeable differences in colour.

Donaldson[15], who also uses the ellipsoids, has a more complex problem, since he matches by means of six colours—Red, Orange, Yellow-green, Green, Blue-green and Blue. This means that there is no longer a unique set of co-ordinates for each matching!

Herdan closes his article with a most interesting observation: 'Apart from the practical importance of these results, they have a certain theoretical interest. They show that the "curvature" of chromaticity space is only another way of expressing the scale uncertainty throughout the chart, the unit of distance varying according to the region of the chart and according to the direction.

'This is precisely how Sir Arthur Eddington (1946) in his

posthumously published work "Fundamental Theory", accounts for and explains curvature of space: once the scale uncertainty has been taken into account, there is no longer any use for the mathematical dodge of curvature.'

Space curvature is, of course, a 'formalist' concept. The word 'curvature' should be written, as Herdan first writes it, within inverted commas. Taken at its face value, curved space, whether ordinary physical space or colour space, is meaningless. Gaussian geometry is no more than a device for giving numbers (the g-numbers) which describe the variation of uncertainty in each direction so that the ellipsoids may be constructed properly.

We shall have occasion in a later chapter to discuss this question of 'uncertainty' as we see it at each level of scientific measurement but first we must examine briefly the meaning of measurement by the senses when the just-noticeable-differences introduce an 'uncertainty' of the kind described by Herdan and also we must see what meaning can be attached to measurements of mental entities themselves.

Note added to proof, April 1950.

The last half of the quotation from Burt on p. 55 is no longer apposite, in the light of recent experiments.

CHAPTER VI

Mental Measurements and Psychological Dimensions

ANYONE WHO HAS SERVED on a committee for assessing priorities knows that, despite the principle of Dimensional Homogeneity, we are frequently obliged to compare magnitudes of a qualitatively incomparable kind. There is so much time and money and so many men available: which is of greater urgency, to build these houses or that hospital, to do this research or that? Governments have to decide not only that Defence Research is *more* important than Medical Research but that it is seventy times more important or at least must be given seventy times the financial support.* There may be some broad measure of agreement on such issues but there is also a large measure of 'uncertainty' and the greater the required accuracy or the more comparable the projects, the greater is the disagreement. There is often much hotter argument over the relative merits of two proposed programmes of research, both within the same field, than over comparisons of widely different projects.

Similarly in aesthetics, almost everyone agrees that the 'Air on the G string' is a better melody than 'Pop goes the Weasel' but who is to say whether a Brahms Concerto is a greater work than a Beethoven Sonata?

In some cases, comparative magnitude, though quite well defined, is not permanent; for example the statement '$4.00 is greater than £1' is true at certain times and places but untrue at others.

Money itself is, of course, primarily a means for overcoming

* British Government Research Budget 1947–8. Ratio now much increased (1950).

dimensional difficulties. Who is to say how many hours of my work are equivalent in value to x hours of a miner's work? Or, still more difficult—how can we compare the value of a fortnight at the seaside and a suit of clothes? These things are decided by assessing all of them in terms of money either as the outcome of industrial disputes or in the free market or by agreed planning. Burt[8] (p. 137) points out that when the householder receives an invoice from his grocer he does not argue 'that the pleasures of cheese and chocolate are commensurable neither with each other nor yet with a magnum of champagne and that consequently the addition exhibited on the bill contains "a fundamental flaw" '.

Indeed, classical Economics laid its claim to be a science on the grounds that the dimensional constants were controlled by a natural law and the increasingly obvious fact that this is far from being the case has caused considerable consternation. According to G. D. H. Cole[9] classical economists have been as critical of those who dared to question their alleged dimensional homogeneities as have some of the classical physicists. As Cole puts it:

'Only such things as can be measured directly . . . are allowed by the "scientific" economists within the pale of economic science. Other values, if they exist in an immeasurable form, are non-economic, and cannot therefore enter into the field of economic studies. . . . The "orthodox" economist of the "liberal" school, confronted with such questions, finds . . . that, because the attempt to answer them involves categories and methods which are not recognized in his analytical "science", they must be nonsense questions, to which no rational answers can be found.'

In this Chapter, we will consider a few problems of measurement which arise in Psychology and we shall leave for a later chapter such technological comparisons as the judgment that 'this clay has a higher plasticity than that'.

It will be best to start with the response of a person (or an animal if its response can be suitably recorded) to some stimulus of varying intensity. Weber found experimentally that the variation in stimulus just great enough to be perceived was in general proportional to the physical intensity of the stimulus itself and

Fechner extended this law by making certain assumptions which have been very much criticized.

There are two distinct points to be considered: first, the experimental validity of Weber's law and secondly, the soundness of Fechner's assumptions. I need say little about the first point. The law holds more accurately for some senses than for others but it gives, within specified limits, a fairly true statement of the experimental facts. The second point is much more serious. Fechner not only used in his extension of Weber's law the amount of stimulus but also 'the amount of sensation'. If we write Weber's law as $\Delta E/E = \text{constant}$ where E is the stimulus, Fechner goes on to assume that, at all levels of stimulus intensity, the smallest perceptible increases in stimulus (ΔE) correspond to equal increases in *sensation* (ΔS). This leads to the equation $\frac{\Delta S}{\Delta E} = \frac{C}{E}$.* The next assumption is that these Δ's can be reduced to infinitesimals, so that $\frac{dS}{dE} = \frac{C}{E}$ or $S = C \log E + B$.

Both these procedures have been thoroughly criticized by Bergson[8] and many others. We will not discuss these criticisms in detail except to quote Tannery who wrote:† 'It will be said [by Fechner] for example, that a sensation of 50 degrees is expressed by the number of differential sensations which would succeed one another from the point where sensation is absent up to the sensation of 50 degrees. . . . I do not see that this is anything but a definition, which is as legitimate as it is arbitrary.'

Much more recently, the whole question of 'Quantitative Estimates of Sensory Events' has been studied by a Special Committee of the British Association (1938). The Report consists of a number of articles by individual members and may be briefly summarized as follows:

* Notice that whereas the Weber constant is a numeric, the Fechner constant C has the dimensions of a sensation, whatever they may be! The constant B depends on there being an absolute threshold below which the stimulus produces no sensation.

† Quoted from Pogson's translation of Bergson's book (p. 67).

'There is nothing inherently numerical in the structure of the phenomenal world'—as J. Guild puts it. This statement forms the centre of the picture. And yet in our urge to measure, we insist on putting numbers to things, although (to quote B. Semeonoff) there is no point-to-point relation between the psychological qualities of sensation and their physical correlates. For example, we normally think of the perceived loudness of a tone as a measure of its physical intensity and of pitch as indicative of frequency. But in fact loudness depends on frequency and on the nature of the harmonics as well as on the intensity.

S. S. Stevens[43] has done some very interesting experiments on the perception of loudness, and his treatment was discussed in detail in the Report we are considering. In his experiments, 'subjects' (this is the usual term for those being tested in psychological experiments) are asked to adjust the intensity of a sound to be twice or half as loud as that of a standard note. Moreover, the ears are so linked that a tone in one ear is half as loud as the same intensity of tone when heard by both ears; so that, as an alternative method, the subject can adjust the tone for one ear to equal the loudness of a standard tone using both ears. In this way, a scale of loudness with a unit, the 'sone' is derived, as distinct from the intensity scale as given in 'phons'. According to Fechner's law, the loudness should be proportional to the just-noticeable differences (abbreviated as j.n.d.). Stevens plots graphs of the logarithm of loudness against the logarithm of the sum of the j.n.d.'s and gets something approximating to straight lines, but the slope is not unity, which means that loudness is related not to the j.n.d.'s directly but to a power of the j.n.d.'s. This power-law relation invalidates not the Weber equation but Fechner's later assumption that j.n.d.'s correspond to equal increments of sensation.

Another interesting type of experiment which may be done with either light or sound, is to give the subject two stimuli differing considerably in intensity. We will call them X_1 and X_5. He is then asked to find a stimulus X_3, spaced equally between them. A stimulus physically half way between X_1 and X_5 is now

compared with each of these in turn, the subject thus defining two new intermediate intensities, X_2 and X_4. He is then asked to halve the X_2–X_4 interval in order that the new X_3 so obtained may be compared with his original X_3. If these should agree reasonably well, the usefulness of the scale is fairly established. The results are somewhat conflicting. F. H. Gage, who did the original experiments, believes that experimental agreement is not adequate to justify the use of the scale but the American workers, Stevens and his colleagues, repeating the experiments with what they believe to be improved technique, claim an adequate accuracy. Stevens, in discussing the difficulties in conceiving the measurement of sensations—says 'We do not measure the magnitudes of a sensation, but only of a particular dimension or aspect of sensation within a single sensory modality'.

There is strong philosophical objection to the process of halving intervals between sensations. It would seem to be a process which subjects should be unable to do. And yet in fact it is done with fair accuracy. (We shall discuss a similar situation in a different connection later.) It is suggested that what really happens is that the subject does not compare intensities, but relations between intensities. In placing B midway between A and C, he is really comparing the 'sameness' of the relations AB and BC. He can then compare the higher order relation involved in this comparison (which we may write AB.BC) with other higher order relations. This idea opens up interesting possibilities as to how complex subjective comparisons may be made and will help us when we come to study the question in a later chapter.

The other question, equally urgent, is concerned with how far we can measure mental entities at all and, if such measurements are found to be reasonably reproducible and useful, what meaning can we attach to the measurements.

In the British Association Committee's discussion, Guild claimed that sensations were not truly measurable because they could be shown to be neither A nor B magnitudes in Campbell's classification. It was pointed out, however, that there might well be other things than A and B magnitudes which could truly be

said to be measurable. For instance, sensation might be measured in terms of stimulus rather than in terms of sensation. There is nothing inherently illogical about this. Burt[6] (p. 126) discusses a distinction made by Meinong between the 'distance' between two points and the 'stretch' ('die Strecke'). 'The former is a line, the latter is not. . . . It may be adequate for practical purposes to estimate distance by the corresponding "stretch", the abstract interval by the number of concrete things we can interpolate within it.'

This idea is developed further in the concept of 'distensive magnitude'. Lord Russell, in his *Principles of Mathematics*, points out that 'the difference or resemblance of two colours is a relation, and is a magnitude; for it is greater or less than other differences or resemblances'. Such magnitudes are called 'distensive' as distinguished from 'extensive'. If we were to define time intervals in terms of the number of events we could insert between those limiting the interval (see p. 31) I think that we should have a 'distensive' measure of time.

Since the war, Stevens has commented further on the discussions of the B.A. Committee. He points out the ambiguity of the terms 'intensive' and 'extensive' and proposes four different types of 'measurement', each of which includes the preceding types.

First come the 'nominal' scales which require no more than the determination of equality, typified by the simple use of numerals for labelling people or things. Secondly, we have scales which require the formulation of 'greater or less than' relations, such as the scale of hardness for minerals, scales of intelligence and personality traits, etc. Thirdly, we have scales which require equality of intervals, such as temperature scales as normally defined. (Intelligence testers are striving towards this type of scale); and lastly come the most fully defined scales for which equality of ratios of intervals must be definable and which can be interconverted by a single factor such as the 2.5 which converts centimetres into inches.

Measurement itself is defined as 'the assignment of numerals to things so as to represent facts and conventions about them'.

Following this definition, Stevens certainly believes in the validity of the concept of mental measurements.

I suppose almost everyone knows to-day that we can measure what is called an 'Intelligence Quotient' or 'I.Q.'. This is simply the ratio (multiplied, for convenience, by 100) of a person's 'intelligence', using the word in a very special and technical sense, to that of an average person, or, in the case of a child, an average child of the same age. The I.Q. is got by asking the subject to perform a set of tests or to answer certain questions; and the tests are so selected as to ensure the minimum effect of educational and environmental differences (i.e. to depend on innate mental capacity) and also so that the figure obtained shall remain as constant as possible during normal mental development. The I.Q. is thus *defined* in terms of the ability to do certain tests and it is found that, within not unreasonable limits, the results are independent of the personality of the tester and remain fairly constant during the ageing of the subject. The score is calculated very generally as if each test in the set were of equal significance though this is known not to be the case and an unexpected distribution of correct answers, though not directly affecting the calculated I.Q., suggests to the psychologist some unbalanced condition in the subject.*

Some sets of tests are known to depend on certain aspects of mental ability (such as verbal ability) more than others and for this reason, the sets of tests are often compared by 'factor analysis', a statistical technique which makes it possible to isolate certain 'factors' common either to all the tests or to some of them. There are also factors characteristic of a single set of tests and due to chance errors on specific occasions.

A number of well-defined 'factors' have been established in this way. Spearman's factor 'g'† which seems to be a criterion of general innate mental ability, is the most famous.

* This is a general statement and would have to be modified slightly in a detailed description. Since the standard deviations of the test scores are not independent of age, and since no two tests measure precisely the same things, many people now prefer to quote scores in terms of standard deviations rather than I.Q.'s.

† See apology, p. 56

If we consider a person doing two tests, A and B (or it may be a schoolboy being examined in two subjects), if he does well in one test there is a fair chance that he will do reasonably well in the other, since general ability is likely to help in any test or examination. But it is also clear that his results in some pairs of tests or subjects (for example, examinations in Latin and Greek) would be likely to be more highly correlated than in others, such as French and Music.

It is easy to work out tables of correlation coefficients* between the different tests and the subsequent procedure is best understood

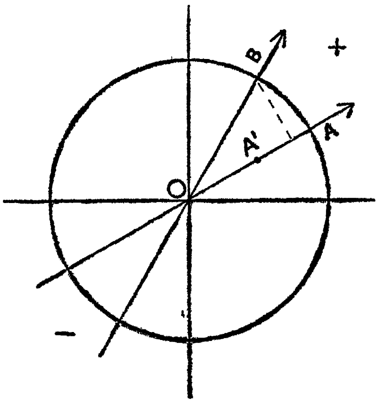


Fig. 5
Illustrating Factorial Analysis

by using a diagram (for a fuller description see text-books by Burt[8], Thomson[44]).

There are a number of quite different methods of analysis. One method is to suppose a person tested to be exactly average in both the tests. We will place him at the point O in Fig. 5.

Now if we draw the straight line OA, we can place other candidates in positions along this line corresponding to their scores above (+) or below (–) the average for test A. I have added arrow-heads to the lines to show that they represent 'vectors' in

* For exact definitions of these well-known statistical terms the reader is referred to any good text-book on Statistics.

which the position on the line indicates the score in the test and the direction of the line, the 'quality' of the test. To indicate what is meant by 'quality' in this sense, we will draw another line OB to represent the results in the second test and we will arrange that the angle between the lines (AOB) shall be a measure of the correlation between the tests.

In order to compare the tests themselves we need some limit to the lines, so we draw a circle of 'unit' radius. The unit which we take for this purpose is the 'standard deviation' for each test, which is derived from the statistical distribution of the results.

More usually, we do not plot the individual samples on the vector diagram but only the tests. Using Fig. 5 now to illustrate this alternative method, if the point for a given test falls somewhere intermediate (A′) between the origin and the periphery, this means that we are really considering the projection from at least one other dimension not accounted for in our diagram, our two dimensions being inadequate to account fully for the data.

The distance AA′, or more strictly $OA^2 - OA'^2$, or $1 - OA'^2$ is a measure of the extent to which other factors ('dimensions') are needed to account fully for the data from the test in question but it also includes the errors inherent in the test itself, which can be independently determined by studying the reliability of replicate tests on similar samples.

But our diagram ought surely to have some axes and these axes, being at right angles, ($\cos 90° = 0$) will be uncorrelated to one another and so represent *uncorrelated factors*. If we construct our diagram by the usual methods, these so-called 'principal axes' are to be found in such a position that no other set of independent factors could exist which would more fully reproduce the results of the measurements.

It is rare for an ability in one direction to be 'negatively correlated' with some other ability—e.g. people who are above the average at languages are not likely to be generally worse than the average in maths. (Even though linguistic ability may not help much with mathematics it is unlikely to hinder, to put it somewhat loosely.) But there *are* cases where negative correlations are found

and psychologists who believe that factors really do correspond to 'abilities' in a quantitative sense, generally eliminate this effect by turning the axes through an angle to get more convenient figures. They may perhaps turn the axes until one of them fits the famous general factor 'g'. So long as the axes are at right angles, the factors are independent of one another but some workers, notably L. L. Thurstone, sometimes allow the angle between the axes to differ from 90°. In some cases it is not suggested that the 'true' factors would really be correlated with one another—the whole point of the treatment is to reduce tests which are 'all mixed up' to a set of factors which are independent of each other—but it is felt that the samples of people tested or the selection of the tests themselves are not truly representative, and it is believed that the use of non-rectilinear ('non-orthogonal') axes corrects for this to some extent.

In other cases, however, the factors are thought of as analogous to such material things as the weight and height of individuals. The size of individuals is best described in terms of weight and height even though these two 'factors' are fairly highly correlated. Thurstone, in his latest book[45] clearly thinks of factors in this sense: Burt[6] and Thomson[44] on the other hand, regard factors much more as arbitrary but convenient mathematical axes.

How many kinds of factors are there? Originally there were supposed to be only two kinds: 'general' (i.e. common to all tests) and 'specific' (related to a single test only). Although it is unlikely that any very hard-and-fast classification is meaningful, we now generally recognize four kinds:

1. Factors common to all tests;
2. Factors common to some but not all;
3. Factors special to one;
4. 'Accidental' factors which depend on the circumstances when the test is given.

If our tests A and B could not be resolved completely into the two 'common' factors but had specific factors as well, the points representing them would not fall on the circle in Fig. 5 but along

the line, as already explained, say at the point A′. (In such cases the correlation coefficient is not simply derived from the cosine of the angle.)*

If we have three factors, we shall need a solid model: for more than three, we must use multidimensional geometry, or stick to algebra, which does not present the same difficulties.

There are many more aspects of factor analysis which do not concern us here but, in connection with our study of measurement, the whole precedure is extremely interesting. For example, Burt[6] is particularly interested in what he calls the process of transposition of factors, which means that instead of comparing n tests from the data obtained with m individuals, we compare the m individuals for each of the n tests.

This has given rise to considerable controversy, W. Stephenson objecting that in some cases, at any rate, we are regarding scores from different kinds of tests as dimensionally compatible when indeed they are not. If we were to score a particular test in a different way, the scores would give different correlations with those of the other tests.

This is exactly the same kind of difficulty that we have already met in so many other connections in this short book. The answer given is that it all depends on whether we regard factors as 'things'.

Burt likens factor analysis to the Fourier analysis of a beam of white light.† Is the red component a real 'thing' or is it no more than a component which could, in this particular case, be separated out by a prism? It would be better perhaps, to take a sound wave for our analogy. Is a particular component of a complex sound wave to be regarded as 'a real thing' or is it no more than a mathematical concept? Dr Silver tells me of a mathematician who performed a Fourier analysis of the profile of a girl's face in order to demonstrate, no doubt, that such a procedure would fail completely to explain the launching of a thousand ships!

* For readers familiar with Vector Theory: the correlation coefficient is given by the scalar product of the pair of test vectors.

† I am told that this example originated with Eddington but cannot find the exact reference.

Elsewhere (pp. 17–18) Burt says: 'When we analyse table salt into sodium, chlorine and a residuum of impurities, we effect an actual physical separation; and we consequently infer that the component atoms or elements are as concrete as the particles of salt. With some such analogy in mind, the student of factor-analysis in psychology is tempted to reify* the factors named, and to visualize a logical analysis as a physical separation, tacitly assuming that, if distinct abilities are ever to be discovered they will be concrete and separate "organs", like the heart or the lungs, and that the "mental mechanisms" which form them will be localized in separate brain centres or cortical areas.

'In psychology however—and, personally, I should add in chemistry as well—what we are really analysing are not substances, but properties: and by properties . . . I understand not attributes inherent in substances, but simply relations manifested under certain constant or standard conditions. . . .

'Our factors, therefore, are to be thought of in the first instance as lines or terms of reference only, not as concrete psychological entities.'

Or, as Thomson[44] puts it: 'I incline to the belief that all so-called "factors", useful as they are in everyday application of psychology, are due to the groupings and the overlapping organization of innumerable small influences both hereditary and environmental, both material and spiritual, and that individual differences are due not to the possession of more or less of this or that "factor" but to a much more complicated set of causes, which, for subtle, (but not mysterious) mathematical reasons which few psychologists have attempted to understand, act "as if" they were a small number of factors.'

Be this as it may, factor analysis could equally well be used in industrial physics to study the inter-relationships between a set of empirical tests on materials;† and in this case, it will be of great

*'Reify' = to regard some concept as a 'thing'. (G.W.S.B.)

† We are now successfully doing this in my laboratory. For work on cheese see Harper, R. and Baron, M., *Nature 162*, 821, 1948, and for plastics, Harper, R., Kent, A. J. and Scott Blair, G. W., *Brit. J. Appl. Phys. 1* 23, 1950.

interest to know whether the factors turn out to coincide with specific 'physical properties' and, if not, whether they could be adjusted to do so by suitable rotation of axes. I think it almost certain that this will prove not to be possible.

Moreover, the vector method of presentation suggests interesting possibilities for quantitative comparisons between things which are apparently dimensionally incompatible and might profitably be used in other connections.

Some psychologists at least are very conscious that if measurements of mental ability, stability and temperament are to be made and used, a theory of measurement analogous to the physical theory of dimensions is needed. In quite early times, Wundt, Titchener and others tried to describe temperamental types in terms of two or three independent dimensions. A most interesting study of this problem has recently been published by H. J. Eysenck.[20]* In discussing the question of the status of factors, Eysenck suggests that there should be 'fundamental dimensions of the mind' comparable to the fundamental units of physics and that if by any chance any of the factors could be shown to be inherited Mendelian units linked with specific genes, they might well be regarded as such dimensional units.

Eysenck has borrowed, in a much modified form, J. S. Mill's distinction between connotative and denotative terms. Mill originally used the term 'connotative' to imply something which has both a subject and an attribute. Thus proper names refer to individual subjects but imply no attributes, whereas qualities (like 'hardness' and 'softness') imply attributes without specifying any particular subject. Mill regarded such terms as non-connotative. The adjectives 'hard' and 'soft', however, imply subjects as well as attributes and would therefore have been said to have connotation.

* For a much more detailed study, which is too difficult for the general reader, see R. B. Cattell's *Description and Measurement of Personality* (Harrap, London, 1946). Some of the ideas which I have quoted from Eysenck's book are, of course, much older and are naturally not claimed by him as original; but, in order to economize in space, I have not referred to earlier sources.

It is in general true that the more we increase the connotative aspects of a term, the less members shall we find within the class we are specifying. This is not a quantitative law, however, since on the one hand, denotation, by which we mean the range of entities falling within our specified class, is not commensurable with connotation, and connotation itself includes attributes which are not of a directly comparable kind.

Moreover, when we increase the connotation of a term, thus giving it greater precision, we generally do so just because an ever increasing number of data has become available for study, so that the range or denotation is not appreciably diminished.

The distinction which we are here trying to make was appreciated long before the time of Mill. Moreover, as time went on it came to be appreciated that Mill's definitions lacked precision and the terms connotative and non-connotative (later, denotative) have gradually changed their meaning. (For a fuller account of this development see my lecture to the Annual Conference of the British Rheologists' Club, Bristol, September, 1949.)

Eysenck's use of these terms is completely different from that of the earlier writers. But the evolution in meaning is not difficult to follow. The physicist, in defining concepts based on a theory, is concerned with the limitations of his postulates, and his concepts are thus thought of as connotative. The craftsman, on the other hand, grouping together all those materials which seem to him to behave in a similar way, concentrates rather on community of behaviour than on specified definitions. Subjective judgments thus come to be classed as primarily denotative. Thus it comes about that Eysenck (quoting Northrop for his authority) uses the term 'denotative' for concepts which depend on something directly perceived or rather abstracted from direct perception, e.g. the concepts of colours: the connotative depend on basic assumptions or hypotheses such as the concept of primary particles—electrons, protons, etc., which are dependent on atomic theory. Isaac Newton pointed out that sensed time and space are not to be confused with what he called 'true or mathematical' time and space, i.e. the time and space of the universe as he postulated it. Sensed time and

space, Eysenck would call denotative and the latter, connotative and he expresses the opinion that statistical factors must be connotative concepts. Now such concepts as intelligence, neuroticism, suggestibility, etc., as we observe them in people are abstracted from our direct observation and are clearly denotative in Eysenck's sense. Herein lies one of the greatest difficulties in developing a psychological theory of dimensions and the position is made more confusing by the frequent use of the same terms to describe both types of concept.

Eysenck's experiments were confined to personnel from the forces suffering from various types of neurosis, hysteria and even psychosis but, though he is most careful not to generalize his conclusions beyond this group of the population, his results are of great general interest.

Since this book is about measurements, I have selected two of the tests which he describes which are of especial interest in this connection. One of them is a test for what is called a type of 'Suggestibility' and the other for 'Neuroticism'. (I hope that the reader will go to Eysenck's book for a full description of the tests which can only be very inadequately described here.)

In the 'sway test' the subject is made to stand quite still with his eyes closed and a thread is attached to the back of his collar by means of a pin. The other end of the thread is fastened to a pointer moving over a scale which indicates back and forward movements of the subject's body, corrections being made for differences in the heights of the subjects. The experimenter, either personally or by means of a gramophone record, suggests to the subject that he is falling forward and in fact he will often begin to sway first backwards then forwards, sometimes even falling over completely. The 'amount of sway' is measured to the nearest half-inch. The subjects, or 'patients' as indeed they were, had been graded into six groups depending on how serious were their neuroses and the groups were then compared with the amount of sway. The results are so striking that I have got Dr Eysenck's permission to reproduce his diagram (Fig. 6).

Let us see how such a measurement as this compares with the

processes of measurement in physics. In the first place, we have a reading of a pointer on a scale, it directly measures a displacement in space; and further, any observer reading the scale would get the same figure for each patient. On the other hand, in so far as the test claims to measure not only 'sway', but through sway, a certain type of suggestibility through the motor effect of the suggestion,

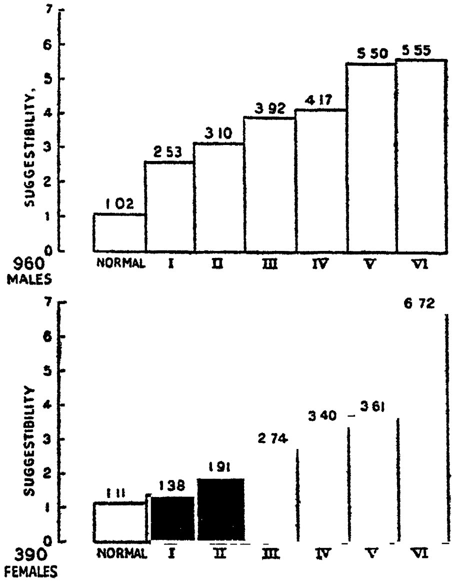


Fig. 6

Average suggestibility (amount of 'Sway' in inches) of Normals and Neurotics showing increase in suggestibility correlated with increase in neuroticism.

From H. J. Eysenck, 'Dimensions of Personality', Routledge & Kegan Paul, 1947.

its status is seen to be quite different, since it is found that the amount of sway is dependent on the personality of the individual giving the suggestion. Although the gramophone record goes some way to meet this difficulty, this is clearly a case where the measurement of response is exact, but where the stimulus can only be imperfectly controlled. Furthermore, in the case of suggestible subjects, previous treatment with a drug, sodium amytal, greatly increased the sway though it did not produce sway in non-

but is significantly related to the mental ill-health of the subjects and 'can successfully distinguish within a hospital population between the more severely neurotic and the less severely ill'.

Here we have then, a useful measurement in which the individual case is regarded as intermediate between the extreme prototypes of health and disease, the measure consisting in the assessing of the position of the individual case in a serial order of numbers. There is no doubt that something very real and very important is being measured—incidentally, almost distinct from both suggestibility and mental ability.

At the end of this interesting book, it is suggested that there may be at least three main 'dimensions' in which mental measurements can be made. These are to be found in the affective sphere ('feeling') as introversion-extraversion, in the cognitive sphere ('knowing') as intelligence, and in the conative sphere ('striving') as neuroticism: these three general factors are nearly but not quite independent of one another. I take it that this means that within each 'dimension', we can hope to make direct comparisons of magnitude—not, of course, that the magnitudes are Campbell's 'A magnitudes' (i.e. which can be added)—but that we can say 'x is more "intelligent" than y'.* We can also say 'x is more introverted but less neurotic than y' and these three statements are independently meaningful and 'true' in the sense that they could be checked within the limits of a definite experimental error, by any competent observer.

We have seen that in physics the choice of fundamental dimensional units is fairly wide. Just as we are not forced to express physical quantities in terms of Mass, Length and Time but can select other units, and numbers of units, so the psychologist must not feel obliged, once he has found a convenient set of dimensional units, to be tied permanently to them. It is my belief that a further understanding of the dimensional situation in the two sciences by all concerned will clarify the problem of both physicists and psychologists.

* I am here using the word 'intelligent' to refer to a measure of I.Q. merely to save time.

CHAPTER VII

The Principles of Uncertainty and of Intermediacy

In our study of measurement we are ultimately faced with three types of limitation. The first and most obvious is that our measuring instrument has a limit of accuracy. We cannot measure the length of a rod with an ordinary wooden ruler to within a thousandth part of a centimetre. This type of limitation is usually indefinite so that as time goes on and techniques improve, we are able to make more and more accurate instruments.

The second type of limitation is fundamental and cannot be overcome by improved technique. It is to be found at at least two distinct levels in physics, though this has not been generally appreciated. It is well-known that at the atomic level, if we are to make any observations of the position or motion of a particle,* say an electron, it must be changed with respect to position or motion by the very radiation with which we may hope to observe it and that the radiation is itself quantized. This means that the more accurately we observe its position, the less accurately shall we be able to record its momentum (i.e. its mass$\times$ velocity). The product of change in position and in momentum must therefore always remain greater than a fixed quantity called 'h' and no amount of refinement in our experimental technique or with the accuracy of our instruments will change the value of h.

The process of measurement itself alters the very things we are measuring and there is nothing to be done about it on account of the quantized nature of radiation and since position and motion

* The Uncertainty Principle is perhaps more easily understood if we describe the electron as a wave rather than as a particle, but this is not so convenient for our line of argument here.

cannot be resolved into simpler terms. The third type of limitation is psychological and we will deal with it later.

At quite a different level of physical measurement, there is another 'uncertainty' which is very similar in some ways to what we may call for convenience 'h-uncertainty'. I have called it 'disintegrational uncertainty' and it is occasioned by the changing of the properties of bodies as a direct result of the processes required to measure those properties. It will be worth while to discuss this in some detail.

We have seen that there are certain groupings of the fundamental dimensional units which are found to be constant during quite diverse physical changes. These constant 'properties' are Newtonian concepts and therefore connotative in Eysenck's sense. They are built up, as we have seen in the illustration of dimensional analysis given in Appendix I, from mass, length and time units, or force, mass, length and time units or indeed from any other suitable set of units. The definitions of the scales of such units are based, as we have also seen, on Newton's laws.

That such groupings of dimensional units would be expected to be constant over a wide range sometimes follows from atomic or molecular theories of the processes involved. But this is by no means always the case, nor, even when it is so does it justify such an extraordinary statement as that recently made by a well-known physicist who wrote, 'We say that we understand a certain class of solid if we can set up a model which explains its characteristic properties'!

Martin Johnson[26] correctly points out that 'by "model" is meant a set of mental images or concepts built on Newtonian laws'. Johnson goes on to say that, 'if symbols however indeterminate can be fitted together in functional form and in equations predicting what actually happens, we regard with tolerant but superior agnosticism our ancestors' desire to label those symbols and attach them to pictures of "things" behaving as little miniatures of large-scale characters or bodies'.

This does not deny, of course, that the concepts which we usually call 'physical properties' may be extremely valuable. As an

illustration of such a 'property', we may take viscosity which, as you will see if you refer to Appendix I, is built up from a force per unit area (pressure) divided by a rate of flow and can therefore be expressed as having for its dimensions either $ML^{-1}T^{-1}$ on the MLT system, or $FL^{-2}T$ on the FLT system.

Viscosity is a useful concept. It is found, for example, that for all simple liquids like water, alcohol, benzene and most of the oils, the viscosity is independent of pressure and rate of flow below the Reynolds' Number at which turbulence starts (see p. 46). Viscosity is not independent of temperature but its variation with temperature usually follows a fairly simple law. Of course, its independence of pressure is only approximate and for very accurate work it is necessary to specify over what ranges of pressure the viscosity is constant enough for anomalies to be overlooked. Nevertheless, viscosity as a 'physical property' is a useful concept.

But there are quite a number of complex materials for which the viscosity is far from independent of pressure. A rubber solution in benzene for example, has a viscosity which falls rapidly as the driving pressure is increased. This does not worry the physicist overmuch since he always finds the same viscosity for any given pressure and he can therefore describe the behaviour of the solution by a graph or an equation showing the viscosities as they vary over a useful range of pressure in much the same way as he must do with all liquids to describe the variation of viscosity with temperature.

But what happens if the very process by which viscosity is measured—dropping a ball through the liquid or forcing it through a narrow tube, for example—changes its structure in such a way that subsequent measurements under the same pressure conditions give quite different results?

This is, in fact, a serious problem in rheology, since in practice many materials behave in this way: some never recover their original consistency while many others do so but slowly.

It might appear at first sight that since we are not here faced with quantum difficulties, all that we need to do is to restrict ourselves to such small disturbances of the system that the effect on

the properties of the material will be negligible. It is true that we can sometimes measure physical properties of complex systems in this way, but it is rarely the case that the property so measured bears any relation to the behaviour of the material as we require to study it in practice.

No doubt the reader will think of similar problems involving non-equilibrium conditions in other fields of physics. It is rightly pointed out that we are trying to measure a property which is being changed by the process of measurement and, perhaps because of the partial analogy with h-uncertainty, many physicists wash their hands of such problems and go and study simpler systems.

This is all very well for the university research worker, but often for the industrial physicist the choice is not so simple. He is obliged to go on working with the material in which his industry is interested and it is incumbent on him to report to his superiors whether physical measurements on the materials in question are justified. Of course if the position were really the same as in the case of h–uncertainty, he would be obliged to explain that there was no possibility of improving simultaneously the accuracy of measurements of whatever was analogous to position and momentum. But in the case of the problems we are now considering, in which we are not troubled by quantum limitations, it is usually found that a number of quite empirical and sometimes accurate and replicable* tests are in use in the industry. The industrial physicist must sort these out and explore their limitations and potentialities but he is also naturally interested to know how to meet the challenge of his academic colleague who assures him that, as processes of measurement, most of his tests are meaningless.

I believe that situations of this kind are responsible for much of the misunderstanding and even distrust prevailing between the 'academic' and the 'practical' man.

The late Professor A. N. Whitehead reminds us in his *Introduc-*

* By 'replicable' as distinct from 'reproducible' I mean that they can be reproduced not on the same sample but on replicates.

tion to Mathematics[47] that Lord Beaconsfield, in one of his novels has defined a practical man as a man who practices the errors of his forefathers; but, although there is some truth in this unkind definition, the 'practical man' also often practices useful arts whose true nature he may not understand but which the academic scientist would do well to study.

Since the correct treatment of behaviour of materials which lose their structure in flow is still highly controversial, I will take another illustration to see if we can find a way out of the impasse produced by this second type of uncertainty which I have called 'disintegrational uncertainty'.

The hardness of metals is often assessed by pressing a very hard metal ball on to the surface of the softer metal to be tested and measuring the depth of the dent left after the pressure is withdrawn. If the metal being tested behaved in a perfectly simple manner like a very viscous liquid, the depth of the indent would be quite simply related to the pressure on the ball and its time of application. In fact, of course, the metal is often hardened by the compression of the ball so that its resistance is increased by the process and it is generally found that if the logarithm of the force is plotted on a graph against the logarithm of the depth of indentation, a straight line is obtained whose slope gives a very useful measure of *hardening* as distinct from *hardness*. Here we are clearly measuring a process by which the 'physical properties' are being changed.

Thus we see that these 'physical properties' are no more than a convenient grouping of dimensional units and that when they are disintegrated by the process of measurement, we can still measure the course of their disintegration in terms of the fundamental units of which they are composed.

To return to our viscosity problem, the viscosity is being changed by our process of measurement, possibly irreversibly, but we can still record the changing relationships between pressure, displacements and time. If we are to recombine these components into more useful units we must remember two things: first we must not forget that they are themselves 'connotative' Newtonian

concepts whereas consistency, hardness (as usually defined) etc., are in their nature 'denotative' in Eysenck's sense. Hence the combinations which will be most stable in a changing process may not be most simply expressed in terms of the usual dimensional formulae based on Newton's laws. Secondly it must be borne in mind that, as Dingle has shown, what is important is not any unchanging 'physical property', but the behaviour of the system undergoing a specific process. This has led some physicists to propose that we should not ask whether a particular material obeys some simple physical law, such as that involving a viscosity independent of changing pressure, but whether under certain specified circumstances the régime under which the law holds is likely to obtain. (See for example F. H. Garner and A. H. Nissan[21].)

A third type of 'uncertainty' lies in the limitations of perception and we have already discussed it briefly in connection with colour matching. At first sight this might seem to be no more than an instrumental inefficiency, comparable with the inaccuracies of our wooden ruler. This is not really the case, however, since we can go on improving the accuracy of our measuring rods whereas we cannot very greatly improve the acuity of our senses. Ultimately, all our scientific observations depend on them and, in the case of denotative concepts like colour the limitations of our visual discrimination form a very real and insurmountable barrier to accurate measurement, analogous indeed to h-uncertainty. (You will not, of course, confuse colour comparisons with measurements of light frequencies after what was said in Chapter V.)

But there is another way in which we may be obliged to break down hard and fast dimensional barriers.

When we measure a length, we say that a six-inch rod is intermediate in length between an inch rod and one which is a foot long. We always do this sort of thing when we compare dimensionally compatible quantities. We do not talk about a foot being intermediate in size between a second and a hundredweight, because these things are not dimensionally compatible.

When we come to psychological classifications, the position is

not so simple. In assessing an I.Q. or an index of neuroticism we are indeed placing our subject between the idiot and the genius* or the normal and the hopelessly neurotic individual. This means that we are regarding differences in such scales as purely quantitative and not qualitative differences. But is the difference between a genius and an average man of the same *kind* as that between an average man and an idiot? Likewise in our colour matching are we justified in regarding chromaticity as a single dimension or are we not in danger of making the mistake which Fechner made in regarding increments of sensation as comparable in magnitude in different parts of the field?

Bergson[3] argued strongly against regarding differences in intensity of sensation as differences of quantity and not quality, and preferred to speak of 'a sensation of increase' rather than of 'an increase of sensation'. He pointed out that, in lifting increasingly heavy weights for example, the whole organization of muscular co-ordination changes.† His point of view has been partly vindicated by later work on the way in which nerve fibres transmit messages. Eddington[18] once described the task of science as being 'to infer knowledge of external objects from a set of signals passing along our nerves'. Neither the magnitude nor the nature of the nerve impulse is altered by the strength of the stimulus, so long as the stimulus is sufficiently powerful to produce the impulse at all.

Dunne[16] (*Serial Universe*, p. 228) has pointed out a type of uncertainty here which is closely reminiscent of h-uncertainty. There is a minimum duration needed to produce a response from a motor nerve as well as a minimum intensity. Hence the product of the minimum stimulus and the minimum duration serves as a kind of quantum (though of energy and not of action) in so far as the nerve is concerned. The size of the quantum varies however, for the different nerves.

* The 'zero' of the scale is the commonest score.

† Bergson's argument that increasing temperature produces a qualitative change from a sensation of warmth to one of pain no longer holds, since this phenomenon is now known to be due to special accessory 'pain fibres'.

Of course, we do directly perceive changes in the intensity of stimuli but this would seem to depend partly on the number of nerve-fibres affected and partly on the frequency with which the nerve impulses are renewed, since only a strong stimulus is able to excite a second impulse in the nerve immediately following an earlier one. (For a popular account of this see W. A. H. Rushton[36].)

Thus our sense of the magnitude of a stimulus depends not on the intensity of an impulse but on the frequency of its repetition and the number of nerve fibres involved. Does this mean that we must not regard the sensations produced by a purely quantitative increase in a stimulus as dimensionally incompatible?

If the reader has accepted my line of argument so far, he will be prepared for my answer to these questions. Incompatibility of dimensions is ultimately a definitional matter. There is a sense in which we can directly compare lengths with times and even with masses. It is all a matter of how we treat our universal constants. Please do not think that I am advocating a unidimensional system for physics—I am not—but if such a thing is possible at all, we must not be too much surprised when we come across these complicated dimensional situations at other and more complex levels of measurement.

What in fact we are doing when we describe some mode of behaviour as intermediate between others which do not appear to be qualitatively comparable is to question the scaling of the fundamental dimensional units.

Supposing, for example, that a complex material does not have a constant viscosity, nor is it perfectly elastic. Neither the rate nor the amount of deformation is proportional to pressure. The classical procedure is to assume that the complex material is made up or behaves as if it were made up of a mixture of perfectly elastic and viscous components* (see my 'Survey of General and Applied Rheology').

This is entirely in the Newtonian tradition. To quote R. G. Collingwood[10] (p. 57), 'In the "*Principia*" a motion that is sub-

* The ratio of the viscosity to the modulus of elasticity is called the 'Relaxation time' (see p. 101).

ject to interference is analysed into two "momenta", the free motion and the motion due to the interfering cause. . . . In the world of actual events, Newton certainly thought that "free motion" occurred only in such combinations; but this is a very different thing from saying that it never occurs at all. If he had said this second thing, he would have built the whole fabric of the "*Principia*" on a breach of his own "*Hypotheses non fingo*".'

I have proposed[88] that in certain cases it is possible to find quite a simple constant which is proportional to pressure by dividing by something which lies between the amount and the rate of deformation. We shall discuss this more fully in the last chapter of this book. Here I would merely remind you that in Chapter III we found that the concepts of constant velocity and acceleration were derived directly from our definition of the equality of time units. If we had selected a different time scale, other entities, now intermediate between velocities and accelerations, would have been described by the whole-number differentials or, being constant, by the simple ratios L/T and L/T^2.

As Thurstone puts it[45] (p. 332) 'The familiarity of mass, momentum and acceleration should not lead us to suppose that they are necessary absolutes for the description of objects in motion. . . . The choice of a set of fundamental concepts in terms of which any domain of nature is to be comprehended, is probably meaningless to nature.'

So when we speak of a constant 'property', i.e. group of dimensional units intermediate between, say, an elasticity and a viscosity, we simply mean that, for this particular complex material, a Newtonian time-scale does not lead to the simplest expression for such groups of units.

This does not mean that in practice we scrap the Newtonian time-scale. On the contrary, to have a separate time-scale for each complex material would be highly inconvenient and too great a price to pay for the advantages of simple dimensional formulae.*

* I am indebted to a critic who pointed out, after a lecture which I recently gave on this subject, that there is a danger that I might be thought to suggest that we are not free to apply any time-scale we wish to any system. If this

We prefer to keep our time-scale at the cost of a more complex conception of 'properties'.

I had already been brought to see the need for such an extension in the formulation of physical properties of complex materials as a result of psychological experiments which we will discuss in the next chapter. In studying psycho-physical processes, it is obvious that we shall not expect to find evidence of inherent Newtonian systems. We may express our concepts in Newtonian terms if we find this convenient but, if we do so, we must realize that we have made a translation into a language which is foreign to the organism which we are studying. It is an 'international' language, no doubt, and as such highly valuable, but it is not the language of the organism.

In concluding this chapter, I should like to add a word about the status of these concepts in relation to physics about which I feel that there has been considerable misunderstanding.*

There would seem to be three possibilities. This concept of Intermediacy between dimensionally incompatible quantities and the resulting Theory of Quasi-properties to which I shall refer in Chapter VIII might be taken either as (1) a contribution to the molecular theory of the solid and liquid states of matter; (2) a convenient way of expressing empirically the results of experiments on complex materials arising specifically from the psycho-physical experiments, or (3) an extension of physical theory which, though showing close relations to psycho-physical concepts, stands in its own right as a new approach to the understanding of phenomena.

I think we are all agreed that (1) is entirely beside the point. These ideas do not add one jot to our knowledge of molecular

point was obscure in my lecture, I hope that my agreement with the speaker's point will be entirely clear in this book. Nevertheless, the use of certain time-scales will enable the behaviour of complex systems to be described in simpler terms than would the use of others. The reader is referred to A. N. Whitehead[47] (Chaps. XII and XVII). [Note added February, 1950.]

* I am indebted to Dr S. Whitehead for explaining to me the nature of this misunderstanding. He is not, of course, responsible for the views expressed here.

processes nor are they intended to do so. On the other hand, I would strongly assert that the study of molecular processes is not the only approach to an understanding even of purely physical phenomena. With regard to (2), I feel that, while it is important that physical theory should not be divorced from psychology, the status of purely physical dimensional theory has been misinterpreted by some physicists and that a review of it would have been worth while even of there had been no possibility of psychological experiments. My own view then corresponds to the third possibility. But I feel that the link with psychology is too important to lose and I shall therefore attempt to explore this aspect of the question in the next Chapter.

In conclusion, it is worth mentioning that in refuting what are, in fact, dimensional criticisms against transposition of factors, Burt[6] (pp. 135–6) makes a remark which clearly shows the connection between factor analysis and the Principle of Intermediacy:

'The very object of factor-analysis is to deduce from an empirical set of test measurements a single figure for each single individual which will plant him on a linear scale for one of a number of independent classifying principles, although each principle of classification embodies a highly complex system.'

CHAPTER VIII

The Theory of Quasi-Properties and Gestalt Psychology

'Die ganzen Zahlen hat Gott gemacht: alles Andere ist Menschenwerk.'—KRONECKER

WE HAVE SEEN THAT the Principle of Dimensional Homogeneity is based on the postulate that we cannot compare quantities unless the things which we are comparing are sufficiently alike in quality for it to be possible for us to find a single 'measuring rod' common to all of them. Thus we can compare a foot with an inch by means of a foot rule divided into twelve inches which are 'equal in length' in the sense that, if the ruler were cut up into pieces an inch long, the pieces could exactly cover each other. Likewise we can compare a minute with an hour by means of a clock such as a pendulum for which we *define* the interval between the beats as equal, with 60 such intervals as one minute and 3,600 as one hour. But we can only compare an inch with a second if we are first prepared to agree on some common ground, e.g. a second could be defined as the time taken for light to travel a certain fraction of an inch or we might define a moderate space of time as a minute (60 seconds) and a moderate distance as a foot (12 inches) and then equate these to one another. 'Moderate' would then mean 'about the middle of the range with which we normally have to deal'. (The number of such definitions will, of course, specify the number of our fundamental dimensional units.)

This last suggestion may sound very foolish—and yet I believe that some process not unlike this may well form the basis of what we may call 'Dimensionally heterogeneous comparisons' which occur so often in everyday life and in the assessment of quality of

industrial materials but which present such a problem to the classical physicist.

I once set out to ask as many people as I could persuade to answer me, a series of questions of which the first ran something like this:

'I want to know your spontaneous reaction to the relative sizes of certain things. Which seems to you the larger, an elephant or a second of time?' The answer was usually 'an elephant', whereupon I asked: 'What about comparing the elephant with a minute or an hour?' and then: 'What time would you equate to the size of an elephant?' Later, I asked for comparisons between lengths and times. Naturally, the narrower I pressed the limits, the more difficulty the subjects found in answering. Hardly anyone had any difficulty in answering the first question.

I suppose that with the elephant and the second, there can be no doubt that the subconscious line of thought must have run something like this: 'An elephant is larger than most animals, whereas a second is small for a time.' Or perhaps as my friend Dr Silver suggests, 'An elephant is much larger than I am, and a second is shorter than most of the periods of time with which I am directly concerned.' We often project such ideas on to other smaller creatures—for example, an ant seems to be travelling very quickly when it covers a distance many times its own length in a short space of time and it is said to carry 'big loads' when the loads are large compared with its own body weight. But if we may use the term 'average animal' in a very general sense, in order to make the equation dimensionally homogeneous, we could use the physicist's dodge (p. 48) and write $\frac{A}{A_o} > \frac{t}{t_o}$ where A_o is the size of an average animal and t_o of an average time.* But as I implied before, this is not very meaningful unless 'average animals' and 'average times' are clearly characteristic of their respective groups, and unless the scaling can be defined in terms of 'standard deviations'.

* The symbol $>$ means 'is greater than'.

I have described experiments very fully elsewhere which my colleagues and I have done on measuring the just noticeable differences in the viscosity of very viscous liquids (bitumens) and in the hardness (elastic modulus) of samples of rubber. The former of these materials is called a liquid because it flows at a steady rate under a constant pressure. If the subject squeezes two samples of bitumen between the fingers (one in each hand),* he can judge which is the firmer by comparing the rates at which they flow. With the rubber, a steady pressure produces a constant deformation, so that firmness can be compared by estimating static amounts of compression.

Mach[81] realized long ago that velocities are judged directly and not analytically from lengths and times. As an illustration he points out that, if the intensity of illumination of a surface is a function of two directions at right angles on the surface (x and y), not only the illumination itself but the rate at which it changes with respect to x and y and the rates at which these differentials themselves change, 'find their expression in sensation'. A line is perceived not only as a succession of points but also its direction and curvature are felt—a point which Bergson also stressed.

But the 'thresholds', or j.n.d.'s, for a simple constant velocity turn out to be some three times as great as those for the static judgment of deformation so that it seems that, even in cases of physical homogeneity of dimensions, acuity of judgment depends on the dimensional complexity. In comparing rubbers, a good subject has an acuity of judgment which is so high that it is sometimes difficult to make physical measurements which are sufficiently reproducible for satisfactory comparison.

When we come to compare a rubber in one hand with a bitumen in the other, the classical physicist finds himself in a dilemma, since he cannot say that an elastic modulus is greater or less than a viscosity. Indeed a number of physicists have advised us against trying to do such experiments. In fact few subjects who are not physicists have any difficulty in making the comparison. Only now we must control the time during which the squeezing takes place,

* Precautions are taken to allow for right- and left-handedness.

since it is found that, for a pair of samples in which rubber is given as softer than bitumen for a short time of squeezing, the reverse judgment is given after longer periods. In fact when a quantitative comparison is made, it is found that consistency and time are just equally important.

Proceeding on the hypothesis that this is because there is one 'T' difference between the dimensions of a viscosity ($ML^{-1}T^{-1}$) and an elasticity ($ML^{-1}T^{-2}$) we have gone on to compare rubber or bitumen for firmness with more complex materials for which we have postulated an 'intermediate' time dimension, for example time might matter half as much as consistency for one material and a quarter as much for another.

In any given time, the materials first investigated by us, all compressed to an extent which was almost exactly proportional to the applied pressure. But there are materials, like the potter's clay, which will hardly deform at all under very small pressures (were this not so, they would not maintain a moulded shape before firing) but which mould easily when greater pressures are used.

We see, then, that to compare the firmness of two very different materials we need to specify at least three magnitudes. The first tells us how far the material will deform for any arbitrary time under any arbitrary pressure, and the other two tell us how the amount of deformation varies with changing pressure and with changing time during which pressure is applied. These three magnitudes taken together give us a full description of the behaviour of many materials, other factors such as temperature being kept constant.

The firmness of an ideally elastic material is described by a simple 'physical property'—its elasticity modulus,* which has definite dimensions ($ML^{-1}T^{-2}$) on the ordinary basis. For a viscous fluid, firmness is defined by viscosity ($ML^{-1}T^{-1}$) which simply means that it is these groupings of mass, length and time which remain independent of pressure and time. For complex materials different groupings are invariant, nor can the groupings

* For the technical reader—I am assuming isotropy and incompressibility.

be expressed in whole number units of M, L and T if we keep the Newtonian definitions of mass and time. Moreover, the fractional powers of M and T (if we fix the power of L as -1) will be different for different materials.

Are we justified in calling one or all of the three constants, which are together needed to describe the behaviour of a complex system, 'physical properties'?

I have asked a great many physicists this question and have received three quite different answers.

(1) 'Certainly not, you shouldn't be working with such things at all—it isn't physics!'

(2) 'Probably better not—these entities are certainly useful and appear to serve as a valuable link between physical tests and the subjective handling judgments made by craftsmen in industry. But a "physical property" ought to have invariable dimensions. Call them something else.'

(3) 'The concept of "physical property" is not really meaningful—these things are as much "physical properties" as anything else. Since we still use the term for convenience, by all means use it for these new concepts as well.'

In view of these three answers, it seemed best to me to follow the middle course and I have decided to call these 'Triads' and other similar groups of magnitudes by the name of 'Quasi-properties'.[38] In some cases, quasi-properties will hardly differ from dimensionally simple 'physical properties'. For example, a big group of plastics has for their constant 'property' an entity whose dimensions hardly differ from $ML^{-1}T^{-1 \cdot 8}$ and the 'firmness' of such plastics can be compared almost as directly by means of this 'property' as can that of elastic solids by their moduli. Clearly this is a matter of degree—how constant is the quasi-property, and how constant do the powers of M and T remain under different processes of test?

At the other end of the scale there are less well-defined 'quasi-properties' which are only reproducible when exactly the same

test processes are used. But these are, nevertheless, often of practical use in industrial testing.

Not all materials obey the particularly simple type of intermediacy which I have just described. There are other types of quasi-properties but the theory has so far only been developed for one branch of physics—rheology*—and, as this is not a book on rheology I will not pursue the subject any further here.

To recapitulate then, the complex behaviour of materials is often most simply specified by a group of magnitudes, all but one of which are numbers describing the intermediate position of the material 'between' dimensionally incompatible prototypes, the one exception (which I call the 'Intensity Factor') having dimensions which depend on the other magnitudes.

This is no more than a useful device for avoiding using non-Newtonian time and force scales. (Time and force are not really on a par here—this difficult question is treated in a paper by G. W. Scott Blair and J. E. Caffyn[39].)

I have been uncertain how to describe these 'groups' of magnitudes since they are not 'groups' in the technical sense of the methematician. I owe to my friend Dr Paul White the excellent phrase 'number triad' when there happen to be three magnitudes, but we are not happy about 'polyad' as a general term. My colleague, Miss M. Baron, suggests inventing a technical use for the word 'Multitude'—a multiplicity of magnitudes—a suggestion which appeals to me.

These 'multitudes' then, are essentially 'wholes' which are more than the sum of their parts, indeed the parts cannot be added, being dimensionally incompatible. When they are pulled apart, the intensity factors by themselves are meaningless, a fact which has misled a number of physicists into condemning the whole scheme.

Psychologists, on the other hand, are well used to such concepts and I was at first tempted without further ado to equate the

* There is some recent evidence that a similar situation is coming about in relation to molecular volumes and surface tensions. (See Scott Blair, G. W., J. Amer. Chem. Soc., *71*, 2950, 1949.)

quasi-property with the psychologists' 'Gestalt'. We now know that this is something of an over-simplification, since the quasi-property is certainly a Gestalt, whereas the effective Gestalten are sometimes quasi-properties and sometimes rather more complex groupings. (Somewhat wider and correspondingly less deep than Gestalt psychology is General Smuts' 'Holism'.[41] It is a pity that Smuts' very great activity in other spheres has not made it possible for him to bring 'Holism' up to date.)

In order to perform any complex action, such as a stroke at tennis, a figure on the ice or the skilled craftsman's handling of his materials, use must be made of 'organized masses of experience' which are not consciously recalled and do not result in any exact repetition of movement learned on earlier occasions but which set the general tone of the action.

Professor Sir F. C. Bartlett has studied these masses of experience under the title of 'schemata'. In many ways they resemble what we might describe (to use a contradiction in terms) as 'acquired instincts'. Like instinctive behaviour the general pattern is defined but not the precise mode of action.

But quite apart from schemata, there are claimed to be innate tendencies, not only in man but in animals, to perceive situations as wholes. In a now famous experiment, very young chicks, which could not have acquired much 'experience', were conditioned to choose the darker of two grey boxes by arranging that food should always be available in this box but not in the lighter box. When the chicks had been so conditioned, they were given a choice of the box in which they had previously been rewarded with food and a box of a still darker grey. The chicks went into the darkest box, showing that they had been conditioned not to a particular shade of grey but to a situation comprising the relation between the shade of the two boxes. They reacted to this situation as a whole.

Such a 'whole', which is more than the sum of its parts, is called by the German name 'Gestalt' which means 'shape' or 'form'. There are many kinds of Gestalt—optical, aural, tactual and, it is claimed, 'physical'. Of these, the optical Gestalten have been the most fully investigated. The reader is referred to text-

books by K. Koffka[28] and W. Köhler[29] as well as an excellent abridged English translation of most of the classical German papers which has been made by W. D. Ellis.[19]

A very simple illustration of obvious aural Gestalten is our recognition of a tune even if it is played in a variety of keys. The individual notes mean nothing taken by themselves, but the grouping and phrasing of them is meaningful even if the notes themselves are of different pitch as they are in the transpositions. Indeed in music, an analytical appreciation as distinct from a Gestalt appreciation requires long training. Even to pick out the individual instruments in an orchestra takes considerable practice.*

Rhythm not only requires a very exact sense of Newtonian time but also the capacity to vary that time in a way which is inextricably interwoven with variations in sound intensity. It would be almost impossible to analyse rhythm into strict 'tempo' and loudness variations, even for a percussion instrument.

The classical papers on Gestalt Psychology were published in the early nineteen twenties and yet the ideas underlying the Gestalt concept can be found in much earlier writings. The following remarkable sentence written by Bergson[8]† as early as 1889 illustrates this: 'If we interrupt the rhythm by dwelling longer than is right on one note of a tune, it is not its exaggerated length, as length, which will warn us of our mistake, but the qualitative change thereby caused in the whole of the musical phrase.'

Some Gestalt psychologists claim purely connotative concepts (in Eysenck's sense) as Gestalten, e.g. Köhler claims that the structures of static electric charges on conductors of given shape are physical Gestalten. Certainly there are many denotative Gestalten of a physical kind, such as firmness of materials as judged by handling, tackiness of such materials as inks and paints and, in the optical field, gloss of paper.

V. G. W. Harrison[22] has written a most interesting little book on this last subject. The conception of gloss is closely bound up

* Adrian suggests that 'a dog can perhaps analyse the pattern of a smell as some of us can analyse the sounds produced by a full orchestra'.

† Pogson's translations (p. 100).

with the way in which light is reflected from surfaces. If we take as prototypes a perfectly reflecting mirror on the one hand, and a completely diffusing surface* on the other, all real surfaces have properties intermediate between the two. Harrison concludes that the Gestalt properties are commercially of greater importance than any group of physical measurements into which we may attempt to analyse them. 'If we are not careful . . . we may lose by the very process of analysis the property we are trying to study. . . . In buying and selling materials, the customer is not in the least interested in physical properties, but in appearances, and for this reason, when there is contradiction between physical measurements and appearances, appearances are the deciding factor.'

When the baker judges the 'body' of his dough as he kneads it, he uses four main criteria, according to Katz[27]: stickiness, elasticity, toughness or firmness, and ductility. Both touch and vision play their part and especially in the case of the former, the effect of temperature is important. 'The experienced baker is not controlled exclusively in his judgments by the actual sense-impressions which he gets in handling the dough; but innumerable earlier experiences of similar and other doughs come into play and contribute to his final judgment.' The Gestalten are modified by the schemata, in fact. Indeed, in studying craftsmanship† it is almost impossible and I think unnecessary to try to distinguish between innate Gestalten and acquired schemata. My colleague Dr Coppen and I, coined the term 'secondary Gestalten' for these more complex entities but this is, of course, very unorthodox Gestalt psychology.

Some Gestalt psychologists believe that the structure of the Gestalt is immediately linked with physico-chemical processes in the brain in much the same way that Hoagland regards the subjective sense of duration to be controlled. As Köhler puts it: 'any actual consciousness is in every case not only blindly coupled with its corresponding psychophysical process, but is akin to it in essential physical properties. . . . The motion of the atoms and

* For technical readers—a surface for which Lambert's cosine law holds.

† See my article in *Penguin Science News* No. 7, 1948.

molecules of the brain are not fundamentally different from thoughts and feelings, but in their molar* aspects, considered as processes of extension, identical'. The body processes co-ordinated to mental Gestalten must have a similar structure to them. This theory is known as 'Psycho-physical Isomorphism' and closely linked with it is Wertheimer's Law of Prägnanz or Fitness, which postulates that the complexity of a Gestalt will tend to run down to the lowest level consistent with the circumstances. This is the psychological counterpart, or, as isomorphists would claim, 'aspect', of the law of minimum free energy in physics.

It is borne out, I think, by the way in which thresholds, or j.n.d.'s for firmness are apparently related to the dimensional complexity of the situation. The firmness-Gestalt, which is doubtless more complex than the corresponding quasi-property, depends for its complexity on the nature of the material, and the thresholds are related to the degree of complexity.

We are only at the beginning of the psycho-physical study of these Gestalten. This is not the place to speak of their importance in the industrial testing of materials but I will close this chapter with a very apt quotation from W. Köhler's book *The Place of Values in a World of Facts*'[30] (p. 393).

'The physicist's hostile attitude towards man', Köhler says, 'will sooner or later have to give way to a wider conception of his own task. His principal preoccupation with regard to man will then no longer be that of immunizing physics against the virus of human subjectivity; rather he will recognize that the functional and structural characteristics even if such subjectivity represent facts of nature which he must include in his system. This will happen when he begins to ask himself: What is the origin of those "subjective" characteristics in a world in which, so far as function is concerned, my data and my principles have a strictly universal significance?' Fortunately not all physicists have 'a hostile attitude towards man!'

* i.e. large scale as distinct from 'molecular'.

CHAPTER IX

The Extension of Physics

> 'Confronted by the data of experience, men of science begin by leaving out of account all those aspects of the facts which do not lend themselves to measurement. . . . Pragmatically they are justified in acting in this odd and extremely arbitrary way; for by concentrating exclusively on the measurable aspects of such elements of experience as can be explained in terms of a causal system, they have been able to achieve a great and ever increasing control over the energies of nature. But power is not the same thing as insight and, as a representative of reality, the scientific picture of the world is inadequate for the simple reason that science does not even profess to deal with experience as a whole, but only with certain aspects of it in certain contexts. All this is quite clearly understood by the more philosophically minded men of science. But unfortunately some scientists, many technicians and most consumers of gadgets have lacked the time and the inclination to examine the philosophical foundations and background of the sciences'—Aldous Huxley.[25]

WE HAVE SEEN THAT time is unidimensional and unidirectional—that is to say, having defined our scale and our origin, we only need one magnitude to define a period of time and events 'move' forward in time and never backwards.

Now if we are to look at a stretch of time, we must move away into a second dimension in order to 'see' it. If you are in a monorail car at rail-level, you can't see any of the length of the rail. Hence to measure or even to scan time, we have to turn it into space. We can follow Mr Dunne[16] in using more than one 'time dimension' if we wish; but this in itself involves ascribing spatial characteristics to time.

And there is a second difficulty which was very clearly discussed by S. Augustine[40] (p. 219): 'We compare one period of

time with another. . . . We measure how much one is longer than another and say that it is double, or triple, or single, or simply that one is as long as the other. But it is time actually passing that we measure by our awareness; who can measure times past which are now no more, or times to come which are not yet, unless you are prepared to say that that which does not exist can be measured?'—(Yes, this quotation is from S. Augustine and not from Bergson!)

True time, or 'durée' as Bergson calls it (the English translation 'duration' is misleading) cannot be measured, and yet for practical purposes we *must* measure time and this is done, as we have already seen, by spatializing it.

The present, which is the only 'time' which 'exists', since the past has ceased to exist and the future has yet to happen, is infinitely short, since any moment can be divided into two parts only, the part which has gone and the part which has yet to come. We have so often heard this puzzle! The catch lies, of course, in the verb 'exists'—which is a present tense. All that this impressive sentence means is that past and future are not present and that the present has no linear dimension—in fact, time is not space.

Moreover, it is undoubtedly true, as James puts it, that the specious present is *psychologically* 'no knife edge but a saddle back with a certain breadth of its own' which implies a direct consciousness of the immediate past.* S. Augustine suggested that the past might be said still to exist as memory and, although the idea of memories as 'traces' in the brain is discredited, it is undoubtedly true that the mechanism by which we perceive the instantaneous ('specious') present must persist in such a way as to give us a direct consciousness of a 'period' of time which must include the immediate past. Miss Cleugh's book on Time[8] goes into this question fairly fully.

Now if we are to get outside time to have a look along the

* There is a sense in which time is quantized but the quantum is far too small to have any direct bearing on our discussion here. Dr S. J. F. Philpott's work, however, suggests that the time quantum may play a part as a unit in controlling the fluctuations in human output.

length of it, and move, with Mr Dunne, into a second time dimension, we shall then want to move into a third time dimension to examine the time 'area' and so on, resulting, as Dunne says, in an 'infinite regress'. (Only I cannot agree that we need only worry about the first two terms. The first term of a series is unique in not having terms on both sides of it but the second term is also unique in having a unique (first) term on one side of it—and so with all the other terms.)

The same situation arises if we paint a picture of the Universe and insist on including a picture of ourselves painting the picture —and so *ad infinitum*.

Again, if we try to 'see' light, we must postulate something by which light is seen, since light is itself a postulate to explain our seeing other things and then we must postulate a means by which we see the means. . . . If we try to observe ourselves making a scientific observation another regress follows. We observe our apparatus 'moving with the "now" ' as Dunne puts it.

Now I agree with Mr Dunne in his objection to the condemnation of these regresses as, of necessity, absurdities. They are not in themselves absurdities, but they inevitably arise if we look at things in this particular way.*

It is debatable, however, in what sense the observer can be regarded as part of the observed. To follow Dunne's analogy, how are we to include ourselves, or at any rate our eyes in our pictures, since we cannot see our own eyes unless by the trick of a mirror? I call it 'the trick of a mirror' for this reason: nothing in the Universe is static, so Mr Dunne's artist's picture must show events everywhere as they are perceived by the 'artist' at a given moment of his time, t_0. Thus events in a star x light-years away will have taken place x years before they are recorded and this 'time lag' will diminish as we approach the location of the 'artist'. If he really could include a picture of himself painting the picture, that part would be 'contemporary'. But if he paints himself from a mirror, the mirror must be a finite distance x' *cm* away from him

* Dunne has also given an interesting explanation of h—uncertainty on serialist lines which I would recommend to the reader's attention. (See p. 82).

and the picture of himself which he records must be $2x'/c$ seconds 'out of date' (c is the speed of light). The artist as depicted in the mirror is, therefore, not in his true temporal sequence in the picture. The same difficulties arise when we try to observe ourselves observing, or to 'see' light. I do not regard these things as absurdities but I do think that they present a serious difficulty to Serialism and it is from them that the infinite regresses arise.

But to return to my main theme, my point is this: If there is to be any science at all, we are *obliged* to turn time into space. Very well! We accept that this is mere symbolism—then why should we worry overmuch when Bergson (and many others much more recently) very correctly point out that sensations are not magnitudes because they differ in quality and not in quantity and that Psycho-physics is 'a symbolical interpretation of quality as quantity'? The whole thing is symbolical anyway. As Burt[6] puts it 'now it appears that the numbers and other quantitative devices which the psychometrist has introduced are in the first instance merely symbolic expedients, employed to help him to state his arguments in a more precise and rigorous form'.

Likewise, the 'equating' of time and space (or of mass and space in General Relativity) is no more than formalist symbolism. The selection of our number of fundamental dimensional units has no more 'realist' status than the rest, though as we have seen, one choice may be more helpful and more meaningful than another.

We are now beginning to see some of the limitations of dimensional theory and I would summarize the ways in which dimensional difficulties are met as follows:

In order to compare magnitudes which appear to be incompatible, we can follow one of the following courses, though they are not mutually exclusive nor rigidly divided from one another:

(1) We can find a universal or ideal phenomenon linking the magnitudes or we can define the scaling of one of them so as to establish a universal relation and then regard the dimensional constant as a numeric. (Examples: regarding a unit of time as 'equivalent to' the distance travelled by

light or a free Newtonian body in that time: eliminating mass as a fundamental dimensional unit by regarding G as a numeric.)

(2) We can express the magnitudes as ratios of some chosen magnitude in the same dimensions. (Examples: psychological test scores as divided by standard deviations, stimuli in terms of j.n.d.'s, or, in physics, postulating a simple process or complex set of simple processes and expressing times as a ratio of the 'relaxation time' (see p. 83).

(3) Occasionally we can find a common 'class' to which both magnitudes belong as when we add cabbages to onions and describe the sum as 'vegetables'.

(4) We can ascribe 'values' to the magnitudes in a common medium such as money. This is convenient but it does not really mean that we regard the original magnitudes as equivalent except for the special purpose of exchange.

(5) Using the Principle of Intermediacy, we can postulate a series of entities of intermediate dimensions. This is easy mathematically. Logically, it means that we realize that the scaling of at least one of our magnitudes is not the simplest for the purpose of comparison but do not wish to alter it for good general reasons. (This is much the same as regarding our magnitude distensively instead of extensively, I think.)

The only superposable measurements are space measurements, hence, as Bergson foresaw, there is an increasing tendency to eliminate the other fundamental dimensional magnitudes. We have seen that times are only superposable when they are already identical. Hence the only way to measure time is to spatialize it. Then why not define the speed of light as a numeric and express times directly as lengths? Likewise with mass: if we are to measure mass spatially why not follow Burniston Brown and write G as a numeric? But surely if we do this, we should hardly retain c as a dimensional constant? This, I am sure, is the direction in which

physics is travelling and it is really all very honest. Perhaps it shows more honesty than wisdom!

But surely a mere psycho-physicist has little to worry about in this new set-up. It will not shock him to be told that his experiments are only 'a more or less rough estimate of the number of sensations which can come in between two given sensations'. Bergson struck that blow in 1889, but the psycho-physicist, though knocked unconscious at the time, has not yet been counted out. Indeed Bergson's description of the transition from one level of sensation to another reads rather like a more modern description of the passage of an electron from one atomic orbit to another.

Only, as we have seen, the psycho-physicist must not expect the entities of interest to him to follow the simple dimensional pattern of 'physical properties'. He need not worry, the physicist is already beginning to doubt whether the 'physical property' is a useful concept at all.

After all, the psycho-physicist is doing just what the classical physicist does only at a different level and his 'uncertainties' are not so insuperable.

As Lancelot Whyte[49] has put it: 'The object set in space and formed of matter follows the necessity of quantitative law. Here is the world of permanence, precision and clarity. With these instruments man emancipates himself from his treacherous subjectivity. He hypostatizes, or establishes as real entities, the permanent features which he has abstracted from process, dividing nature in order to master it.'

Our dimensional difficulties remain so long as we are at the integrated level. If we look at the process and not at the permanent features abstracted from it, dimensional difficulties do not arise.

For example, if we differentiate our equation on p. 47

$$y = A\,x^n$$

we get $\dfrac{dy}{y} = n\,\dfrac{dx}{x}$ which is a dimensionless equation. In practice, of course, we have to deal with integrated abstractions rather than with processes—hence the dimensional difficulties. If we wish to compare quantities of things which differ in quality, we can either

ignore the quality difference or, if that is impossible we can, as in factor analysis, regard the entities as vectors representing quality. This is really another application of the Principle of Intermediacy and a very similar process has been used in rheology ('The Nutting Square'—see my *Survey of General and Applied Rheology* and my article in *Brit. J. Philos. Sci.* 1, 1950).

Now surely we begin to see how this throws light not only on psycho-physical concepts but on the comparison of complex physical magnitudes. Again, as with measurements of duration, we are forced to do the impossible. We are forced to say that 'this clay is more plastic than that' though we know that the plasticities are not qualitatively compatible. Of course, the potter does it every time —but that is by comparing his sensations— a mere 'symbolical interpretation of quality as quantity'.

The physicist has three choices—no, really two only, since he cannot for long pass by on the other side saying that 'this is not pure science'. Either he can maintain his dimensional integrity and forego the Newtonian scale definitions or (and this I am sure is the more sensible course) he can keep the Newtonian definitions and express the behaviour of his materials in terms of 'multiplicities of magnitudes'.

For the present this would appear to mean using fractional differential equations but I am not sure whether therein lies the true answer to Wertheimer's request for a Gestalt mathematics or whether the fractional differentials will prove to be no more than a temporary expedient to be left behind when a better mathematical tool becomes available.

Indeed, Burt[6] (p. 239) suggests that 'to the contemporary psychologist who believes that conscious phenomena are best described in terms of the patterns or Gestalten that they display, rather than in terms of atomistic sensations held together by "laws of association", matrix algebra should obviously appear the appropriate quantitative tool'. He goes on to point out, however, that even matrix algebra has its disadvantages since 'we are still condemned to cross-multiply and add'. Perhaps the fractional differentional 'multitude' is better off in consisting of magnitudes which

are dimensionally incompatible and cannot be added: perhaps Burt's later suggestion that the Theory of Groups* holds the key to our problem will prove correct. I do not claim to know.

We are here concerned, of course, entirely with the phenomenological aspects of the behaviour of materials and largely with what Eysenck would call denotative concepts. It is curious that these directly perceived aspects of the physical world are often regarded with the greatest suspicion, 'reality' or objectivity being supposed to reside only in the connotative concepts.

As Köhler[30] puts it (p. 169): 'the "macroscopic" or "molar" aspect of the physical world is sometimes regarded almost as an illusion. Modern science is atomistic. We find at present most of experimental and mathematical physics concerned with such entities as molecules, ions, atoms, electrons, protons, neutrons and so on. Knowledge of these particles and their behaviour may easily seem to be the only goal of science. The particles themselves will consequently appear to be the only "real" content of nature. . . . Influenced by this attitude of the expert the layman is apt to give less importance to that vast body of physical knowledge which had been built up before the era of atomic research. What was the characteristic of such knowledge, since it could not yet be truly atomistic? It was, as a matter of experimental experience, wholly macroscopic.'

Are we, then, to extend physics (and indeed science as a whole) to include both the molecular and the 'molar', both the extensive and the distensive, both the universal and the merely 'general'?

Burt[6] (p. 121) discusses the suggestion that 'it might be better to base our criterion of what is, or what is not, the subject matter of science, not on agreement between experts,† but on agreement with the facts, i.e. power of prediction'. 'Then', Burt says, 'I may add that the psychologist's prediction of what a child will do in

* 'A set of operators such that the product of any two of them always gives an operator belonging to the set is called a group'—Eddington[18] gives an excellent elementary account of the Theory of Groups. Fractional differentials, as defined in Appendix III, have the properties of an infinite group.

† I take it he means 'those trained to make the observations'.

certain tests tomorrow is far more accurate than the meteorologist's prediction of what tomorrow's weather will be.' No doubt—yet it still remains true that the astronomer's prediction of what the sun will be doing in a million years time is far more accurate than any psychologist's prediction of what Mr Bevin and Mr Molotov will be doing in six weeks!*

The point surely is that *all these matters have to be studied.* Just where we define the frontiers of physics or of science as a whole, hardly matters—unless we allow our definitions to become our masters and demand passports and visas for the crossing of the frontiers.

No doubt it is true that it is dangerous for a chemist to wander too far into the realms of physics, psychology and philosophy—perhaps my critics will find me a sinner in this respect—but, however unworthy a traveller I may be (and I am deeply conscious of my defects) I maintain that scientific, like international relations, will only be improved when people cease to be afraid to cross frontiers.

* Written in April 1948.

Appendix I

(*see p.* 45)

Dimensional Derivation of Stokes' Law

A. Using the ordinary fundamental units, Mass, Length, Time:

We have for the velocity:

$$V \propto r^a .\ \Delta\rho^b .\ g^c .\ \eta^d$$

$$LT^{-1} = L^a\ [ML^{-3}]^b\ [LT^{-2}]^c\ [ML^{-1}T^{-1}]^d$$

$$\left.\begin{array}{lll} \text{solving for M:} & 0 = b + d \\ \quad\text{,,}\qquad\text{,,}\ \text{L:} & 1 = a - 3b + c - d \\ \quad\text{,,}\qquad\text{,,}\ \text{T:} & -1 = -2c - d \end{array}\right\}$$

If we assume (see text) that b=c,

$$\left.\begin{array}{r} c = -d \\ a - 3c = 1 \end{array}\right\}\ \begin{array}{ll} \text{Hence} & c = 1 \\ \quad\text{,,} & d = -1 \\ \quad\text{,,} & a = 2 \end{array}$$

hence $V \propto r^2 .\ \Delta\rho .\ g/\eta$.

B. Using as fundamental units M. L. T and F (force) we have:

$$LT^{-1} = L^a .\ [ML^{-3}]^b\ [FM^{-1}]^c\ [FL^{-2}T]^d$$

$$\left.\begin{array}{lll} \text{Solving for M:} & 0 = b - c \\ \quad\text{,,}\qquad\text{,,}\ \text{L:} & 1 = a - 3b - 2d \\ \quad\text{,,}\qquad\text{,,}\ \text{T:} & -1 = d \\ \quad\text{,,}\qquad\text{,,}\ \text{F:} & 0 = c + d \end{array}\right\}$$

$$\text{Hence:}\quad \left.\begin{array}{l} d = -1 \\ c = 1 \\ b = 1 \\ a = 2 \end{array}\right\}$$

hence again: $V \propto r^2 .\ \Delta\rho .\ g/\eta$.

Appendix II

(*see p.* 56)

On the use of Gaussian co-ordinates

In an ordinary map we rule squares north and south, and east and west, in order to describe the position of the places on the map. A part of such a map is shown in Fig. 7 (a).

If we want to know the distance between A and B, we first measure how far east we have to go from A in order to get due south of B—i.e. 4 miles in this case; and then how many miles north we must go to reach B—i.e. 2 miles.

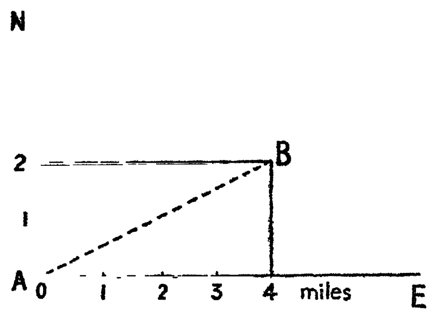


Fig. 7a

Now from Pythagoras' famous theorem which we probably learned at school, we may remember that the length of AB is given by $AB^2=4^2+2^2$ or $AB=\sqrt{20}$, about $4\frac{1}{2}$ miles.

But suppose that the map represents not flat but hilly country—or even, to take a simpler case, that the distance between A and B is so great that the earth's curvature has to be taken into account. Instead of having straight lines ruled at right angles, we shall have to have a series of curves, as I have shown quite roughly in Fig. 7 (b).

If we make our squares small enough, we can suppose that the N–S and E–W lines (which we will now call x and y axes) are as

near as no matter straight for any single square which we are considering, though of course they will not be at right angles but at some other angle as I have shown at B.

Now it can be shown that instead of our simple Pythagorean equation to relate the very small distance AB (which we will call ds) to dx and dy, we must use a slightly more complicated equation:

$$ds^2 = g_{xx}\,dx^2 + g_{yy}\,dy^2 + 2g_{xy}\,dx\,dy.$$

where g_{xx}, g_{yy} and g_{xy} are simply numbers specifying the amount of curvature. (The suffix 'xx' means, for example, the number associated with the dx.dx relation and likewise for xy,

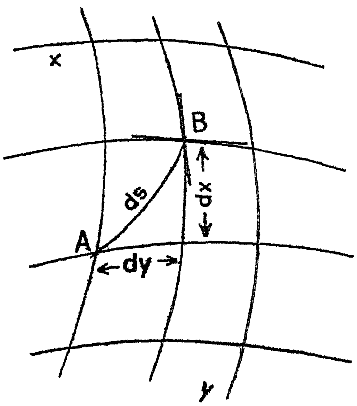


Fig. 7b

Diagrams illustrating Cartesian and Gaussian Co-ordinates.

etc.) If we straighten the lines out so that we get back to the right angles of Fig. 7 (a), then g_{xx} and g_{yy} must become 1 and g_{xy} becomes 0, so that we again have the simple Pythagorean equation.

Now although we can picture a curved surface but are not able to picture a curved space, there is no reason why we should not develop an exactly similar equation for use with our solid model instead of with our curved map. We now have, three directions, x, y and z, and so we can write:

$$ds^2 = g_{xx}\,dx^2 + g_{yy}\,dy^2 + g_{zz}\,dz^2 + 2\,g_{xy}\,dx\,dy + 2g_{yz}\,dy\,dz + 2\,g_{xz}\,dx\,dz.$$

This equation, which can be represented by an ellipsoid, gives us six numbers which are needed fully to describe our 'curved space'.

Another way of looking at this is to suppose that at any point in the space, we need to know the amount of curvature in the x, y and z directions and also three more numbers to tell us how these three curvatures are changing as we go in the x, y and z directions. It is clear that the change of the x curvature in the y direction is not to be distinguished from the change of the y curvature in the x direction. This is why we have only three g's relating different letters (xy, yz and xz) and why they are multiplied by 2. Now these six numbers enable us to fix the lengths of the three axes of our ellipsoid and also their positions in space.

I have drawn the curves in Fig. 7 (b) almost as if they were radii of two circles. This makes the picture rather easier to understand, but Gauss' treatment holds perfectly well whatever the shape of the curves provided only that they do not cross other members of their own series.

Appendix III

(*see p.* 84)

Note on Fractional Differentiation

What is the meaning to be attached to an entity intermediate between a length and a velocity and how is it to be expressed mathematically?

We can define fractional differentiation to mean what we want it to, but the most useful definition for our purpose will be to define n $1/n^{th}$ differentiations as equivalent to a single differentiation.*

In other words, to take an example, $\frac{d^{1/3}l}{dt^{1/3}}$ implies an operation on l which, when done three times, is equivalent to the operation described by $\frac{dl}{dt}$.

Let us be quite clear at the outset that it is the *operation* which is intermediate—not the values obtained from the operation. These may be, and sometimes definitely are not, intermediate between the corresponding arithmetic values of l and of dl/dt.

In order to use these fractional differential expressions, we express the numerical coefficient as a quotient of gamma functions.

The gamma function, which is a kind of 'fractional'equivalent to the factorial, is defined in this way:

$$\Gamma(k+1)=k\,\Gamma(k)=\int_0^\infty e^{-x}x^k\,dx$$

Now consider the equation

$$l=A\,t^k$$

for ordinary differentiation we have

$$\frac{dl}{dt}=k\,A\,t^{k-1}$$

* This is not a complete 'definition'.

or, differentiating n times:

$$\frac{d^n l}{dt^n} = A.\ k\ (k-1)\ (k-2)\ \ldots\ (k-n+1)\ t^{k-n}$$

$$= A\ \frac{\underline{|k}}{\underline{|k-n}}\ .\ t^{k-n} \text{ assuming } k > n \text{ and both integers.}$$

Now suppose n is a fraction, we can write:

$$\left|\overline{\frac{d^n l}{dt^n}}\right. = A\ \frac{\Gamma\ (k+1).}{\Gamma\ (k-n+1)}\ t^{k-n}$$ where $\left|\overline{}\right.$ indicates that the differentiation is fractional.

Empirically it will be seen that the gamma function has the same form as the factorial except that 1 is added.

In the special case where n=k, these equations reduce to:

$$\frac{d^k l}{dt^k} = A\ \underline{|k}$$

and
$$\left|\overline{\frac{d^k l}{dt^k}}\right. = A.\ \Gamma\ (k+1)$$

The values of $\Gamma\ (k+1)$ and of $\Gamma\ (k-n+1)$ are found, for specific numerical values of k and n, from a table of gamma functions.

We have thus defined a fractional differentiation—but it must not be forgotten that the *significance* of this fractional differential expression lies simply in the fact that it obviates the necessity for using a time-scale (i.e. definition of equality of time intervals) characteristic of the particular system studied. All fractional differentials could be eliminated from our treatment if we were willing to forego the Newtonian definitions of the fundamental dimensional units. But to do this would be far more nuisance than the fractional differentiations. We should still require magnitudes to define specific time and force scales just as we now require them as exponents of the fractional differential expressions.

REFERENCES

[1] ALEXANDER, W. P., Brit. J. Psychol., Monograph Suppl., No. 19. 1935.

[2] BARNES, E. W., Scientific Theory and Religion. *Cambridge Univ. Press* 1933.

[3] BERGSON, H. Essai sur les Données Immédiates de la Conscience. *Alcan*, Paris, 1889. (Trans. entitled 'Time & Freewill' by F. L. POGSON, *Swan Sonnenschein*, London, 1910.)

[4] BRIDGMAN, P. W. Dimensional Analysis. *Yale Univ. Press*, 1922.

[5] BURNISTON BROWN, G. Proc. Phys. Soc. *53*, 418. 1941.

[6] BURT, C. The Factors of the Mind. *Univ. of London Press*, 1940.

[7] CAMPBELL, N. R. Measurement and Calculation. *Longmans Green*, London, 1928.

[8] CLEUGH, M. F. Time. *Methuen*, London, 1937.

[9] COLE, G. D. H. An Intelligent Man's Guide to the Post-War World. *Gollancz*, London, 1947.

[10] COLLINGWOOD, R. G. Metaphysics. *Oxford Univ. Press*, 1940.

[11] DINGLE, H. Science and Human Experience. *Williams & Norgate*, London, 1931.

[12] DINGLE, H. Through Science to Philosophy. *Oxford Univ. Press*, 1937

[13] DINGLE, H. Nature. *144*, 888 (1939): *145*, 427 (1940); *146*, 391 (1940)

[14] DINGLE, H. The Special Theory of Relativity. *Methuen*, London, 2nd Edn., 1946.

[15] DONALDSON, R. Proc. Phys. Soc. Lond. *59*, 554. 1947.

[16] DUNNE, J. W. An Experiment with Time. *Faber & Faber*, London, 1927.
The Serial Universe. *Faber & Faber*, London, 1934.

[17] EDDINGTON, SIR A. S. The Nature of the Physical World. *Cambridge Univ. Press*, 1928.

[18] EDDINGTON, SIR A. S. New Pathways of Science. *Cambridge Univ. Press*, 1935.

[19] ELLIS, W. D. A Source Book of Gestalt Psychology. *Kegan Paul*, London, 1938.

[20] EYSENCK, H. J. Dimensions of Personality. *Kegan Paul*, London, 1947.

[21] GARNER, F. H. and NISSAN, A. H. Nature, *160*, 329. 1947.

[22] HARRISON, V. G. W. Definition and Measurement of Gloss. *Patra*, London, 1945.

[23] HERDAN, G. Brit. Sci. News. *1*. (4) 12. 1947.

[24] HOAGLAND, H. Pace Makers in Relation to Aspects of Behavior. *MacMillan*, New York, 1935.

[25] HUXLEY, A. Science, Liberty and Peace. *Chatto & Windus*, London, 1947.

[26] JOHNSON, M. Time, Knowledge and the Nebulae. *Faber & Faber*, London, 1945.

[27] KATZ, D. Cereal Chem. *14*, 382. 1937.

[28] KOFFKA, K. Principles of Gestalt Psychology. *Kegan Paul*, London, 1935.

[29] KÖHLER, W. Gestalt Psychology. *Bell*, London, 1930.

[30] KÖHLER, W. The Place of Value in a World of Facts. *Kegan Paul*, London, 1939.

[31] MACH, E. Space and Geometry. (Trans. T. J. MCCORMACK.) *Kegan Paul*, London, 1906.

[32] DU NOÜY, L. Biological Time. *Methuen*, London, 1936.

[33] PIÉRON, H. Presidential Address to French Assoc. for Adv. of Sci. 1945.

[34] POINCARÉ, H. La Valeur de la Science. *Flammarion*, Paris, 1904.

[35] PORTER, A. W. The Method of Dimensions. *Methuen*, London, 1933.

[36] RUSHTON, W. A. H. Penguin Science News, *5*, 123. 1947.

[37] SCHRÖDINGER, E. What is Life? *Cambridge Univ. Press*. 1946.

[38] SCOTT BLAIR, G. W., VEINOGLOU, B. C. and CAFFYN, J. E. Proc. Roy. Soc. *189a*, 69. 1947.

[39] SCOTT BLAIR, G. W. and CAFFYN, J. E. Phil. Mag. *40*, 80. 1949.

[40] SHEED, J. F. The Confessions of Saint Augustine. *Sheed & Ward*, London, 1945.

[41] SMUTS, J. Holism and Evolution. *Macmillan*, London, 3rd Edn, 1936.

[42] STEVENS, B. The Identity Theory. *Sherratt & Hughes*, Manchester, 1936.
The Psychology of Physics. *Sherratt & Hughes*, Manchester, 1939.

[43] STEVENS, S. S. Psychol. Rev. *43*, 405. 1936.

[44] THOMSON, G. The Factional Analysis of Human Ability. *Univ. of London Press*, 1939.

[45] THURSTONE, L. L. Multiple Factor Analysis. *Univ. of Chicago Press*, 1945.

[46] WERTHEIMER, M. Address to the Kant Society, Berlin, 1924. (Quoted from W. D. Ellis, Ref. 19.)

[47] WHITEHEAD, A. N. An Introduction to Mathematics. *Oxford Univ. Press*. Home Univ. Library, 1944 Edn.

[48] WHITTAKER, SIR E. Space and Spirit. *Nelson*, Edinburgh, 1946.

[49] WHYTE, L. L. The Next Development in Man. *Cresset Press*, London, 1944.

[50] WRIGHT, W. D. Researches on Normal and Defective Colour Vision *Henry Kimpton*, London, 1946.

Index

THIS BOOK IS SET *in 12-pt Fournier, a letter based on the original design by Pierre Simon Fournier. Fournier was the first of many famous type-designers of the eighteenth century to cut complete families of type, large- and small-faced romans, condensed, bold, and italic series, shown in his type specimen book published in 1742,* Modèles de caractères. *The invention of the point system of type measurement is ascribed to him.*

The Monotype version used in this book is based on Fournier's medium text types, one of the narrowest book faces available to-day. The accompanying italic version is a graceful letter, differing in its fundamental conception from other italic types.